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The Adventures of Luis Alvarez: Identity Politics in the Making of an American Science

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The Adventures of Luis Alvarez:
Identity Politics in the Making of an American Science

by

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Dedication

To my parents, Rubén and Angelina, for not keeping a language of secrets.

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In the 1930s and 1940s, American atomic physicists developed an identity akin to those ethnic identities developed by Chicanos and African Americans in the 1960s. Tremendous successes in high-energy physics put these American physicists at the pinnacle of science worldwide. Luis W. Alvarez was one of the central figures in this rise, was central to the development of "Big Science," and won the Nobel Prize in 1968. However, historians have largely ignored him. Through Alvarez we see that American atomic physicists before the 1930s lacked an identity. Alvarez witnessed the growth of his field and was an early advocate for an identity for American atomic physicists. Using identity politics as a theme, we find five stories centered on Alvarez that illustrate this

emerging self-image. Alvarez's autobiography demonstrates his interest in preserving the history of physics and establishing his place in it. A textbook draft that Alvarez abandoned in 1952 further illustrates his early interest in the history of physics then absent in physics textbooks and an early interest in mythology and heroes. Alvarez's work outside of physics helps define the boundaries of this newly self-identifying group as he conquered fields like forensics and pyramidology, as well as famously proposing the theory that an asteroid killed the dinosaurs. A collection of letters from cranks helps us demarcate science from non-science and thus define the boundaries of science. Finally, Alvarez's identity as a physicist is contrasted with another category of identity, his ethnic identity. Alvarez was a white man with a Hispanic name, which provides us with the rare case of a white man discussing his whiteness with would-be biographers who wanted to frame him as a "Chicano physicist." Altogether, Alvarez would, much more than any physicist in his generation, promoted and exemplified an identity as an American atomic physicist while rejecting other identities.

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Preface: ***How I Came to Write About Alvarez***

I chanced into the story of Luis Walter Alvarez and the development of an American physics identity. While developing a dissertation topic, I noticed that much of my writing had been about a cultural shift in American physics, a new assertiveness, and a new self-image. It seemed a natural fit to explore whether identity politics could apply to a group like physicists or whether it only worked for ethnic and racial groups. My research suggested that it was possible and Alvarez soon emerged as a focal point for many aspects of what could define such an identity. He was extremely concerned in the way he would be remembered and helped develop much of the new style of physics exemplified by the University of California's Lawrence Berkeley Laboratory.

I was a physics major at U.C. Davis, but I was stymied because I never became comfortable with differential equations, so I settled for a minor in quantum mechanics. Luckily, I discovered Chicano studies and American history, and eventually the history of physics. In graduate school, I socialized more often with physics graduate students than with those in history. That is part of the reason I make an effort to work in as many stories as possible from Feynman's *"Surely You're Joking, Mr. Feynman!"*—I share their heroes. I have found the history of physics immensely rewarding.

I expected that I would go into Chicano history, as it, and *The Autobiography of Malcolm X*, attracted me to history. Before historian Alex Pang introduced me to the history of physics, I focused largely on Chicano identity politics. What did it mean to be

Chicano? What was the definition, and how did it differ from Mexican American or Hispanic or Latino? Richard Rodriguez's *Hunger of Memory* got under my skin for being so eloquent on identity, yet using these concepts to arrive at a conclusion I rejected.[1] In my first semester of graduate school, I read E. P. Thomson's tremendously influential *The Making of the English Working Class*, a book that introduced bottom-up history.[2] For me, that book expanded my notion of identity beyond race or ethnicity. All the ideas were in place for something like this dissertation, but I needed a subject, a character on which to apply these ideas.

In my senior year of college, I read Thomas Hughes's *American Genesis*, which I enjoyed greatly. Almost in passing, he mentions Alvarez to contrast the style of physics done at Berkeley and Chicago, where Alvarez was a graduate student.[3] In the summer before graduate school, I thought I had better read a few books, especially Bruce Hunt's *The Maxwellians*. I checked the footnote in *American Genesis* to find out more about this Luis Alvarez. Perhaps I could do Chicano identity and the history of physics at the same time! The footnote led me to Alvarez's autobiography, in which he stated that he was white on the first page of chapter one, after the prologue. In drafts, Alvarez explained his racial background on the first page, but his ghost writer, Richard Rhodes, moved the Hiroshima story to the beginning, using a literary device he elsewhere described as *in*

[1] Richard Rodriguez, *Hunger of Memory: The Education of Richard Rodriguez* (New York: Bantam Books, 1982).
[2] E. P. Thomson, *The Making of the English Working Class* (New York: Vintage Books, 1963).
[3] Thomas Hughes, *American Genesis: A Century of Invention and Technological Enthusiasm, 1870-1970* (New York: Penguin Books, 1989), p. 406.

medias res or "in the middle of things"—a hook to get the book started.[4] So much for combining Chicano studies and the history of physics, I thought.

In graduate school, I discovered whiteness studies by reading David Roediger's *The Wages of Whiteness* in Neil Foley's class on race and ethnicity.[5] I spent two summers in Berkeley to find out what the Luis W. Alvarez papers contained. That research became a class paper on Alvarez's whiteness, and then I moved on. Eventually, I realized that Alvarez was the key to understanding identity politics in American physics. I made countless short trips to Berkeley to fill out the picture I had started with my whiteness paper. Alvarez was an early proponent of physicists' self-awareness, of heroes in physics, of developing an identity for American physics, even if he did not use the terms I use.

To explore this, I would need to define my terms. Alvarez was an experimentalist working primarily on hydrogen bubble chambers, but his identity seemed to reach beyond that narrow category. His hero was Ernest Rutherford, a New Zealand-born British experimentalist who demonstrated that the atom has a nucleus in 1911 (American physics did not have the history yet to provide him with a homegrown hero, it seems). Alvarez worked closely with experimentalists working on the particle accelerators and with some theorists investigating subatomic particles. He worked on the atomic bomb at Los Alamos. What this group had in common, despite the changing names over time,

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[4] Richard Rhodes, *How to Write: Advice and Reflections* (New York: Quill, 1995), p. 51.

[5] David Roediger, *The Wages of Whiteness: Race and the Making of the American Working Class* (London: Verso, p. 1991).

was that they worked on the physics of the very small. Although it is not a term used by physicists today, Alvarez was an American atomic physicist. That was his identity.

Goals and Results

I began with two premises to get to my thesis. The first is simply that Alvarez is understudied by historians, the second is that I would like to tease out value from seemingly difficult sources. Obviously, Rhodes's *The Making of the Atomic Bomb* featured Alvarez as the experimentalist who solved the problem of simultaneously discharging the thirty-two detonators in Seth Neddermeyer's implosion design.[6] However, Rhodes agreed that historians should examine Alvarez further. After all, the Manhattan Project was one small part of his career. One example of Alvarez being underutilized is J. L. Heilbron and Robert Seidel's impressive *Lawrence and His Laboratory*.[7] The 523-page tome on U. C. Berkeley physicist Ernest Lawrence appropriately focuses on his legacy: the Radiation Laboratory, now called the Lawrence Berkeley Laboratory. Alvarez was a central figure in the laboratory's success, but he is mentioned on only twenty-seven pages, half of which is about work on neutrons that was not central to Alvarez's career. Daniel Kevles's *The Physicists*, probably the best survey of the development of the American physics community available, mentions Alvarez once.[8] Only Peter Galison's *Image and Logic* addresses Alvarez's role at the Lawrence Berkeley Laboratory in any detail in a lengthy chapter titled "Bubble Chambers:

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[6] Richard Rhodes, *The Making of the Atomic Bomb* (New York: Simon and Schuster, 1986).
[7] J. L. Heilbron and Robert Seidel, *Lawrence and His Laboratory: A History of the Lawrence Berkeley Laboratory, Volume 1* (Berkeley: University of California Press, 1989).
[8] Daniel Kevles's *The Physicists: The History of a Scientific Community in Modern America* (Cambridge, Mass.: Harvard University Press, 1995).

Factories of Physics."[9] This dissertation will not compete with *Image and Logic* on Alvarez's accomplishments at the Radiation Laboratory. Instead, this cultural history will describe broader trends in the physics community using Alvarez as a vantage point, not a focus.

Galison had one advantage in the one source I do not have: he interviewed Alvarez on 7 March 1983. Alvarez died when I was about to start my junior year of high school. Galison used the Alvarez Papers at the National Archives, Pacific Region at San Bruno, California. I used the copies at the Berkeley Bancroft Library that, according to the finding aid, has the same core collection of seventy-seven boxes. Both have small, secondary collections that include photographs and laboratory notebooks. However, we had entirely different goals. Galison was trying to make the case that particle physics has to decide between two approaches that he labels "image" and "logic." When a particle accelerator generates a torrent of particles, physicists can photograph the particle trails with a cloud chamber or with the bubble chambers that Alvarez developed. Alternatively, physicists could use the counting approach—Galison's "logic"—used at labs equipped with spark chambers and wire counters. That there was a transition from image to logic is Galison's thesis, and so his use of the Alvarez Papers is appropriate: he looked at Alvarez's manuscripts and correspondence in the late 1950s, boxes one through six and a few widely available manuscripts. This is not a criticism of Galison—Alvarez

[9] Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997).

was not his primary subject—but it is a shame that historians of science have only just touched on such an important figure.

My goal is different. I believe that the Alvarez Papers are a goldmine that has barely been explored by historians. Alvarez is widely recognized as being a central figure in the community of high-energy physicists from the late 1930s to the late 1960s, so should we not see what else his papers include? Alvarez was a pack rat, keeping copies of everything, from every bit of correspondence, inbound or outgoing, newspaper and magazine clippings, and even letters from cranks. He had an interest in preserving the history of science, once noting that self-awareness in a journal article on his efforts to find hidden chambers in an Egyptian pyramid:

> If we ever find anything in the mass of the Second Pyramid, the very extensive file of letters that has passed back and forth between Cairo and Berkeley will become "historical documents" and someone will probably use them as source material in a history of the project.[10]

Alvarez did not find those chambers, but he was right about the historian. Instead of a clear idea of what I wanted to find in the archives, I dove in hoping to find a story. Thankfully, I did.

Alvarez participated in the enormous growth of experimental physics described as "Big Science," as well as the rise of other sciences and relative decline of physics around the time he grew frustrated with his lab work and expanded into novel and curious uses of his skills. What struck me about his experience in the rise and fall of big science is that his identity changed through the transitions. He went from telling people he was a

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[10] Alvarez, "Subsequent Developments," *Adventures in Experimental Physics* α (1972), p. 176. This journal used lower case Greek letters for issue numbers and was only published for two years.

chemist to avoid explaining what a physicist does to being a swaggering adventurer of a physicist. Likewise, he went from the peak of experimental physics a few years before his Nobel Prize to growing uninterested and needing other outlets for his creativity and knowledge of physics, such as working on the Egyptian Pyramids and discovering what killed the dinosaurs. Throughout his archives, I found traces of Alvarez developing an identity as an American physicist in ways that put him ahead of the curve. He was not always the prime example of identity formation among American physicists, but he was one of the first examples in enough fields that he warrants our attention. Robert Millikan, the first American-born physicist to win the Nobel Prize, also stands out as an American physicist who tried to create an identity for American physicists, but he seems to have come too early. Alvarez came at precisely the right time.

Chapter 1: *Alvarez and the Rise of American Atomic Physics*

> It's hard to realize how recently the word 'physicist' came into common usage. In the 1930s, after I earned my physics Ph.D., I usually told laymen who asked that I was a chemist. Otherwise a long explanation would have been required.[1]
>
> - Luis W. Alvarez

Luis Alvarez was right—it is hard to imagine that physics was not well known in the U.S. before the Second World War. The postwar obsession with everything atomic has been well documented both in academia and in popular culture.[2] Even popular culture accepts that physics is part of a tradition going back to Isaac Newton's reformulation of gravity in 1687 or even Galileo's struggles with the church in the 1630s. Children are taught that an apple hit Newton on the head and he "discovered" gravity on Saturday morning cartoons like "Schoolhouse Rock."[3] For his achievements, Newton was knighted and made Master of the Mint. How could it be that Newton was celebrated in his own life but the young Alvarez could not count on his contemporaries to even know what physics was?

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[1] Luis W. Alvarez, *Alvarez: Adventures of a Physicist* (New York: Basic Books, 1987), p. 16.
[2] See for example, *The Atomic Café*. Directed by Jayne Loader, Kevin Rafferty, and Pierce Rafferty. (New York: Docurama Films, 1982).
[3] "A Victim of Gravity," Schoolhouse Rock 30th Anniversary. DVD. (Burbank, CA: Buena Vista Home Entertainment, 1978).

Figure 1.1: Luis Walter Alvarez in 1968.

Physics is the study of matter and energy. Let us get that out of the way now. Luis Alvarez was a physicist at the University of California, Berkeley. He won the Nobel Prize in 1968 for his contributions to hydrogen bubble chambers, the detectors that read the results of particle collisions like those at today's much-discussed Large Hadron Collider near Geneva, Switzerland. Particle accelerators today use wire detectors that can more easily port their data to a computer, but as we shall see, Alvarez was on the cutting edge of computerization before wire detectors. He is most well known in the public sphere for developing a theory with his son that an asteroid killed the dinosaurs. He worked on the Manhattan Project developing the detonators that are now the standard in the explosives industry and was the only person to witness the first three atomic bomb explosions at Trinity, Hiroshima, and Nagasaki. He was an accomplished pilot who was awarded the Collier Trophy for inventing the radar system for blind landing and he helped develop the radar array that made an English victory possible at the Battle of Britain. He had many patents and was inducted into the Inventor's Hall of Fame. He was a witness to a tremendous amount of history. He published an autobiography and he kept meticulous records now filling seventy-eight boxes, three cartons, and thirty-six notebooks at Berkeley's Bancroft Library, but historians largely overlook him.

This is not a biography of Luis Alvarez. It is a cultural history of American atomic physics as seen through Alvarez's career. Historian Robert Kargon captured the idea in the opening to his book on American physicist Robert Millikan:

> I do not consider myself to be Robert Millikan's biographer. This book is not a full record of Millikan's life or even of his scientific career. It is an essay,

> very selective, on themes that are illustrated and illuminated by Millikan's life in American science.[4]

To be sure, Alvarez deserves a biography, but he did publish an entertaining and informative autobiography, *Alvarez: Adventures of a Physicist* in 1987, a year before his death. The story of how that book came to be published is the subject of chapter three, but if the reader wants to read the story of Alvarez's career, his autobiography will be difficult to top. After all, Alvarez's autobiography was ghost written by the Pulitzer Prize winning author of the excellent *The Making of the Atomic Bomb*, Richard Rhodes. I am not likely to top Rhodes's writing. I asked Rhodes if it was odd to write a dissertation on someone who had already written an autobiography; he said, "No, it's not. Autobiographies are full of lies, you know."[5] It is true that an autobiography can be self-serving and that Alvarez might have different goals than a historian, but this work will not attempt to compete with *Alvarez: Adventures of a Physicist*.

Instead of retelling his life and work, I hope to illuminate a trend in the history of American atomic physics that historians have not spelled out. Americans did not widely recognize the achievements or even existence of physics before the Manhattan Project resulted in the atomic bomb. One might ask about Albert Einstein, the theorist who is arguably the most well known man of the twentieth century; was he not a famous physicist since 1919? Surprisingly, Einstein was widely known as a mathematician. The people who acknowledge "Einstein" as a synonym for brilliant do not claim to

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[4] Robert H. Kargon, *The Rise of Robert Millikan: Portrait of a Life in American Science* (Ithaca: Cornell University Press, 1982), p. 11.

[5] Richard Rhodes interview by author, 20 May 2009, p. 39.

understand his work well enough to categorize it into a then-obscure field such as physics. Science education has come a long way since the Second World War and Sputnik.

On the other hand, physics became the premier science after the Manhattan Project. The reasons are no surprise; it turns out that if you give physicists billions of dollars, they can devise a bomb that can destroy an entire city. What must people have thought physicists would give them next, hover cars and death rays? While this is obviously a simplification of the Manhattan Project and its public response, there is clearly no need to investigate how physics became the premier science. Instead, the historian should study how that transition played out, what effects did it have on physicists as a group—besides the obvious and well-documented "Big Science" movement—and what we can learn from the rise and fall of American atomic physics. This is cultural history.

AMERICAN PHYSICS BEFORE THE MANHATTAN PROJECT

American physics did not have a homegrown founder such as Isaac Newton. We came late to the field and struggled to get our footing. Daniel Kevles's *The Physicists* captures that ascent thoroughly. He describes efforts by physicists such as Millikan and others who tried to raise awareness and money for physics. However, American physicists had not accomplished enough to take away from the successes of theorists in Germany and experimentalists at Cambridge, to name two established groups. Furthermore, it is not clear that, even in England, physicists wanted much attention from the general public. Legend has it that a popular toast at Cambridge went: "To the electron, may it never be of any use to anybody!"[6] Cambridge was well established and had no need for the public's attention. Millikan was much more interested in getting whatever attention possible from government, industry, or academia. He succeeded in establishing American physics as a respectable field, but the average American physicist did not develop a public identity. Physics was still under the radar for the general public.

The First World War is often described as "The Chemist's War" for the profound influence those scientists had on battle. Most famously, chemical warfare changed the battlefield in ways that stunned the public—it has "chemical" right in the name. However, it was the refinement of high explosives that caused the most destruction. Chemists, who had already been crucial to the development of materials science, were in the public spotlight well before physics.

The generation of physicists entering the field in the years before the bombing of Hiroshima worked in obscurity. Richard Feynman's highly entertaining collection of essays, *"Surely You're Joking, Mr. Feynman!"* contains two such stories. During the Second World War, Feynman recalled, "You have to understand that, in those days, people hardly knew what a physicist was. Einstein was known as a mathematician, for instance—so it was rare that anybody needed physicists."[7] When he described trying to get a job before the Second World War, he had the same experience, but noted the change after the war:

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[6] There are many versions of this story, but the Public Broadcasting Service credits Cambridge physicist and discoverer of the electron J. J. Thomson for this toast. http://www.pbs.org/transistor/science/events/electron.html

[7] Richard Feynman, *"Surely You're Joking, Mr. Feynman!": Adventures of a Curious Character* (New York: Bantam Books, 1985), p. 84.

> At the time nobody knew what a physicist even was, and there weren't any positions in industry for physicists. Engineers, OK; but physicists—nobody knew how to use them. It's interesting that very soon, after the war, it was the exact opposite: people wanted physicists everywhere.[8]

That is an understatement. Feynman described a request from a general that he evaluate some weapons for the Army. He twice replied that he was a theoretical physicist and did not know anything about weapons. The third request came from the Secretary of the Army, so Feynman finally gave in. At a cocktail party, one "guy in a uniform" asked Feynman, "since the physicists can get energy out of uranium, could I work out a way in which we could use silicon dioxide—sand, dirt—as a fuel?"[9] These were the expectations the public and military had of physicists after the Manhattan Project. It was quite a shift from not knowing what physics was a few years earlier.

HOW TO WIN A NOBEL PRIZE

To the reader who wants to learn about Alvarez's career, I recommend his autobiography. I cannot capture all the stories already told there. However, it is appropriate that I introduce the work that earned him the Nobel Prize with a focus on the difficulties he overcame and his pattern of drifting away from and having to relearn experimental physics. Alvarez won the Nobel Prize in physics in 1968, but many, he included, thought he had no chance after having missed out on the 1960 Nobel Prize given to Don Glaser for the invention of the hydrogen bubble chamber. For all the talk of more and more powerful particle accelerators, energetic particles are of no use to science if they cannot be observed with appropriate equipment. Essentially, a hydrogen bubble

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8 *ibid.*, p. 38.
9 *ibid.*, pp. 262-3.

chamber photographs trails of tiny bubbles showing the path that particles take through it. It works by superheating liquid hydrogen to the point where it wants to become a gas, then at the precise moment particles are shot through it, dropping the pressure in the containing chamber, causing charged ions to bring hydrogen to boiling. Those trails are then photographed with a stereoscopic camera. One can determine the mass and charge of the particles by their curved path as they pass through a magnetic field. That device obviously helped produce a tremendous amount of data and many important discoveries, but it was clear that Alvarez perfected it and did a lot more work with it than Glaser, who left physics for molecular biology in 1962. Many expected Glaser and Alvarez to share a Nobel Prize. Alvarez was devastated by losing it and most believed that once the Nobel Committee passed somebody by, that he would never be considered again. He refocused his energy into physics in hopes of another shot at the prize in a pattern that he repeated throughout his career.

When Alvarez was in graduate school, he described having to play catch up. He was an experimentalist with little experience with theory. He was "determined to atone for my misspent youth [and] the several years of lectures I had neglected to attend."[10] After graduate school, he found that his Ph.D. education at Chicago was "atrocious"[11] and again set out to relearn physics, this time by scouring the library. World War II took Alvarez out of experimental physics from 1940 to 1945; his radar and atomic bomb work were closer to engineering than modern physics. He managed to do some real work as

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[10] Alvarez, *Adventures*, pp. 37-38.
[11] *ibid.*, p. 45.

soon as he got back to Berkeley, helping Edwin McMillan build the synchrotron, developing the Hilac (Heavy Ion Linear Accelerator), and helping Lawrence set up the Livermore lab.[12]

Alvarez and Ernest Lawrence became passionate advocates for the thermonuclear or "H-bomb" around 1950. Alvarez thought it was a necessary defense against the communist Soviet Union and thought he could help. Alvarez spent most of 1950 to 1952 working at Livermore on the MTA, or Materials Testing Accelerator, in order to produce large volumes of material for both fission (atomic) bombs and fusion (thermonuclear) bombs. Had the project succeeded, Alvarez would have found himself running a $5 billion project, but the project was postponed indefinitely in August of 1952.[13] Peter Galison described the project as "Alvarez's MTA." Alvarez described it as "not a happy time,"[14] at least in part because it took him away from pure research. Rhodes said Alvarez realized he had to refocus, "and that's when he stopped working for Ernest Lawrence on all this drivel that he was doing."[15]

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[12] Before World War II, Ernest Lawrence established and worked at the Radiation Laboratory in the Berkeley hills. After the war, he built the Lawrence Radiation Laboratory at Livermore about 50 miles southeast of Berkeley to take over military work, leaving the original Radiation Laboratory to do pure physics. It was completed in 1952. After Lawrence's death in 1958, Edwin McMillan took over the Radiation Laboratory, which was renamed the Lawrence Berkeley Laboratory. The Livermore Laboratory became the Lawrence Livermore National Laboratory in 1971. Alvarez did his Nobel work at the Lawrence Berkeley Laboratory.

[13] Galison, *Image and Logic*, p. 343.

[14] Alvarez, *Adventures*, p. 173.

[15] Rhodes interview by author, 20 May 2009, p. 27.

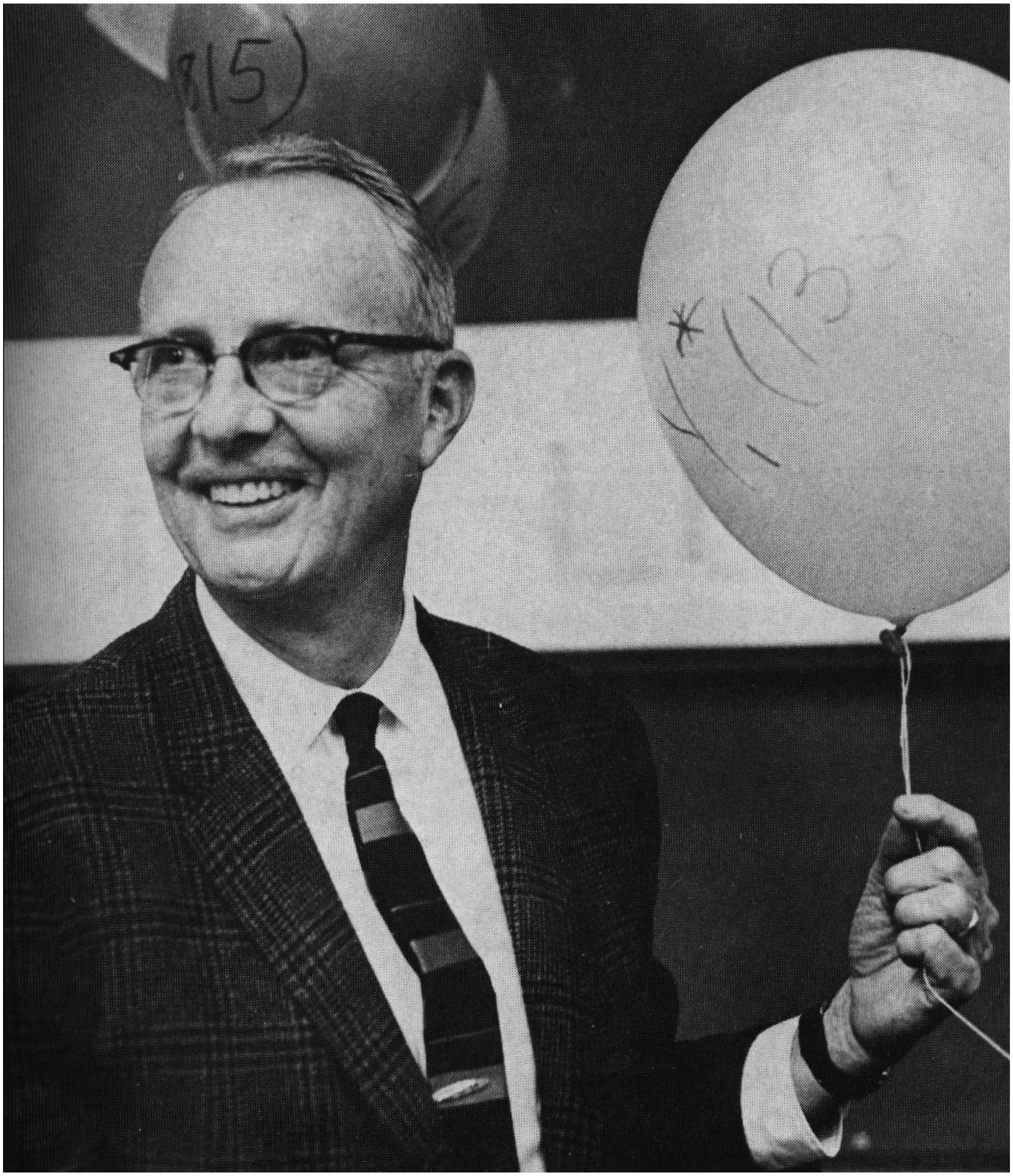

Figure 1.2: Alvarez celebrating his Nobel Prize in Sweden in 1968.

Alvarez found himself an outsider to high-energy physics. Physicist Herbert York left Berkeley in 1952 to head the new Livermore lab at the site of the abandoned MTA, leaving behind two graduate students, Lynn Stevenson and Frank Crawford. Alvarez described his conversation with York about the two students as "one of the best things that ever happened to me [and] led me to find my way back."[16] Alvarez took on York's graduate students, his first in five years, and offered them a deal: "I would hire them as my research assistants if they would treat me as *their* research assistant."[17] Alvarez, now forty, described himself as a "has-been" who, like baseball players, "do their best work when they're young."[18] He recalled, "The experience was hard on the ego but exciting."[19] Alvarez enthusiastically relearned physics once again.

The year the MTA shut down, 1952, was also the year Donald Glaser invented the bubble chamber. Glaser was also at Berkeley and Alvarez worked to improve on Glaser's invention. Glaser's small bubble chamber was not very useful, but Alvarez made it much larger in order to observe powerful collisions. Alvarez jumped back into physics doing the work that would eventually win him the Nobel Prize. Alvarez's primary engineering innovation was a bit sloppy, he admitted. Small bubble chambers had the problem of bubbles forming on any imperfect surface, so they were built out of very smooth glass. Pete Schwemin came up with the idea of a "dirty" chamber that had an imperfect, metal chamber and a plate of glass attached as a window. This way, the hydrogen on the sides boiled, but the center section near the glass remained clear and a metal chamber could be much stronger and larger. Alvarez worked to develop

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[16] Alvarez, *Adventures*, p. 177.
[17] *ibid.*, p. 178
[18] *ibid.*
[19] *ibid.*

Schwemin's 2.5-inch chamber into a four inch one, then ten inches. While Schwemin wanted an incremental increase in size, Alvarez pushed for exponential growth. His most productive chamber was 72" long by 20" wide by 15" deep. That much liquid hydrogen was dangerous; nobody had made a piece of optical glass that size and strength; the size of the project and staff ballooned such that Alvarez needed a chief of staff. Lawrence, who did not live to see the completion of the 72-inch chamber, thought it was too big. Alvarez solved many problems that came with a project of this size and complexity by acting as a manager as hydrogen bubble chambers went from small projects to the work of large teams.

The other aspect of Alvarez's innovation was the development of a system to process the data. Through the window, a stereoscopic camera would take photographs at the moment the particles collided. By 1968, Alvarez was processing 1.5 million events a year.[20] Analyzing that many photographs required computerization. Engineer Jack Franck developed a "Franckenstein" for the five-step process summarized by Galison: a nonphysicist would look for potentially interesting events, a physicist would record rough data, a nonphysicist would use Alvarez's Spiral Reader to enter data points onto punch cards, a computer would fit the data points to a curve, and a specialist would evaluate the data for errors.[21] This large bubble chamber, working quickly, with results processed by computer, revolutionized particle physics and helped discover many new particles.

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[20] *ibid*., p. 191.

[21] Galison, *Image and Logic*, p. 373.

In 1960, Don Glaser won an unshared Nobel Prize in physics. Alvarez was devastated. As he wrote in his autobiography,

> I had been considered by the Nobel committee and found wanting. That is something few physicists ever hear; nor have many seen that judgment reversed.
>
> I've enjoyed my life immensely, but I would not like to relive 1960.[22]

Alvarez tried to get a second shot at the Nobel Prize. Rhodes told me that "Glenn Seaborg and the gang said don't [try again] because typically the Nobel committee only passes over once; doesn't go back and do it twice. He was really proud that they actually reconsidered him for the prize since he had already been passed over once."[23] He had a lot of help, including Glenn Seaborg, who had succeeded Ernest Lawrence as head of the Lawrence Berkeley Laboratory and was a Nobel Prize winner. Alvarez said that Seaborg convinced "the Nobel committee that I should be awarded the 1968 physics prize."[24] Rhodes noted that "all of his friends wrote letters" to the Nobel Committee and that "whatever his character in terms of being abrasive, he obviously had friends."[25] Alvarez had an amazing ability to relearn physics whenever he realized that he had lost his way. This says a lot about Alvarez's energy and persistence, but it also demonstrates that staying abreast of physics is incredibly difficult. Even someone as successful as Alvarez had a very hard time keeping up.

Perhaps it is difficult to keep up with modern experimental physics because it changes very quickly. In an interview conducted in February 1967, over a year before

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22 Alvarez, *Adventures*, p. 214.
23 Rhodes interview by author, 20 May 2009, p. 27.
24 Alvarez, *Adventures*, p. 41.
25 Rhodes interview by author, 20 May 2009, p. 27

Alvarez won the Nobel Prize, he said, "The interesting thing to me is that if I were a graduate student coming into physics now, I wouldn't work in my own group because I would find it all too dull."[26] He lamented that in his own group,

> our people don't go down and look at the bubble chamber very often or at the Bevatron. They ask the bubble chamber operators to expose a certain number of million frames of film, and then they ask somebody else to measure them, and then run them through computer programs, and then they start with computer program output and process this data.[27]

He acknowledged: "I brought the computers in. I didn't do it personally, but if it hadn't been for the big bubble chambers they wouldn't have come—the big bubble chambers and the data reduction techniques that my group and I pioneered."[28] Later, he would try to get away from the computerized work, first in cosmic ray physics, then in fields outside physics altogether. I can imagine what he would think of modern physics "experiments" that do not just use a computer to process data from some piece of laboratory equipment; they are carried out entirely on a computer.

Given the title of this section, I would be remiss not to address Alvarez's strategy for winning the Nobel Prize, but obviously persistence, a strong network of friends, and the ability to manage a large, complex project helped. Luis's father, Walter Alvarez, M.D., was very supportive of Luis's creativity and interest in science, buying the young Luis a crystal radio kit and encouraging him to read from his substantial library. Walter introduced the teenage Luis to University of Chicago physicist Arthur Holly Compton by

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[26] Alvarez interview by Charles Weiner and Barry Richman on 15 Feb. 1967, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, session II, p. 39.
[27] *ibid*.
[28] *ibid*.

simply walking in to his lab and introducing himself and Luis. Walter had been director of the Markle Foundation that gave Lawrence funding for medical research using accelerated particle beams.[29] Luis's older sister Gladys was Lawrence's secretary and mentioned Alvarez to him. After Alvarez finished graduate school, Lawrence offered him a job. After Alvarez was an established physicist, his father gave him some advice. Walter had once nearly discovered a treatment for human pernicious anemia. His immediate supervisor, Dr. George Whipple, discovered the treatment soon after and won the 1934 Nobel Prize in medicine. Walter realized that he had not stopped to think about his work and its potential, instead moving on to other pressing concerns. Luis wrote, "He advised me to sit every few months in my reading chair for an entire evening, close my eyes, and try to think of new problems to solve. I took his advice very seriously and have been glad ever since that I did."[30] As I rush to complete this dissertation, I realize that it does not afford me a chance to think about future directions for research. I look forward to trying Walter Alvarez's technique.

THE LITERATURE

Daniel Kevles's *The Physicists* does provide a similar story, told in a radically different way. He also tells of the rise of American physics in the twentieth century, but his more resembles a traditional history as a series of events. That is appropriate because the story of the development of American physics had not yet been told. Kevles provides an excellent foundation on the development of American physics, even if he largely

[29] *ibid*., p. 15.
[30] Alvarez, *Adventures*, p. 58.

leaves out Alvarez. This dissertation is an attempt to add to the traditional history that has been skillfully told by established historians. The stories I tell here would not make sense if others had not established the foundation.

I obviously relied heavily on two published works, Alvarez's autobiography and his festschrift, both published in 1987. Alvarez's former student, Peter Trower, compiled a festschrift for Alvarez that contained twenty-one articles by Alvarez, each with an essay by someone involved in that work that explains its background and context.[31] Much of it is gushing with praise, as Alvarez had many loyal followers, but it also includes essays by rivals who certainly respected him even if they sometimes clashed. Finally, there are also several children's books on Alvarez that I will discuss in the chapter on his ethnicity. Those books usually played up his Spanish heritage.

There is one book that I feared had "scooped" me: Iwan Rhys Morus's *When Physics Became King*, which "follows the story of physics throughout [the nineteenth century], showing how a science that barely existed in 1800 came to be regarded a hundred years later as the ultimate key to unlocking nature's secrets."[32] However, Morus analyzed the history of English physics, which had a fundamentally different trajectory than American physics. The first clue is apparent in that quote; American physics barely existed in 1900, much less 1800. American physics developed later and did not have the tradition going back to Isaac Newton, who published his *Principia Mathematica* in 1687. Although it may seem repetitive and wordy to keep repeating American physics, it is

[31] Peter Trower, ed., *Discovering Alvarez: Selected Works of Luis W. Alvarez, with Commentary by His Students and Colleagues* (Chicago: The University of Chicago Press, 1987).
[32] Iwan Rhys Morus, *When Physics Became King* (Chicago: University of Chicago Press, 2005), p. 1.

important to keep that distinction clear. A cultural history of physics cannot claim to be vague about which country is under investigation. While they share results and theories with their overseas colleagues, American physics developed in a different environment and in a different century. American physics is in a sense an offshoot of English and German physics, but the cultural history of each is unique.

A very recent book on physics and cultural history frames this work but perhaps misses the importance of physicists of Alvarez's generation. David Kaiser's *How the Hippies Saved Physics: Science, Counterculture, and the Quantum Revival*[33] argues that the decline in physics described by Kevles was reversed by a renewed interest in the philosophy of physics by members of Berkeley's Fundamental Fysiks Group in the 1970s. Kaiser admits to an overreaching title, but makes an interesting case that Alvarez's generation of American physics—although he did not discuss Alvarez specifically—turned physics into a very practical, results-oriented discipline devoid of the big picture explorations of Einstein's generation in Europe. Kaiser's first chapter captures the argument well: "Shut Up and Calculate." I would argue that Kaiser is on to something, but to make his point, he is dismissive of the style of physics Alvarez helped develop. My thesis is that although Alvarez's Big Science approach may have been very practical and less philosophical—Alvarez was an experimentalist, after all—his generation of American physicists gave physics the prestige in America that would allow the Fundamental Fysiks Group to become the darlings of the psychics, mystics, and

[33] David Kaiser, *How the Hippies Saved Physics: Science, Counterculture, and the Quantum Revival* (New York: W. W. Norton, 2011).

hippies. Alvarez and his colleagues made American physics the envy of varied and disparate group, as we shall see in chapters four, five, and six.

In the preface to the first edition of *The Physicists*, Kevles recalled meeting physicist I. I. Rabi "a few years" before that book's publication in 1978. Rabi remarked "that there were few histories of modern science or biographies of its leading figures. Why doesn't someone write about my generation of physicists? he asked in his twinkling manner: 'After all, we changed the world.'"[34] Kevles, Rhodes, Galison, Heilbron, Seidel, and others have gone a long way to correcting that, but the history of American physics is still a young field. We have a lot to cover and that Alvarez's tale has not been adequately explored may mean that it is too early for alternative histories about ethnicity and physics, Nutfiles, and hero mythologies in physics. Perhaps we need more surveys of the developments in modern physics to establish the facts first. We historians are going to have a difficult time understanding some of the more esoteric theories in recent physics. However, given Alvarez's fine autobiography and the work of historians discussed here, it seems like a fine time to attempt to do for physics the type of analysis historians have applied to the Renaissance, Reformation, and English labor history.

STORIES I FOUND

Through my search in the Alvarez papers, as well as his published works, his autobiography, and works by others, five stories emerged. While there are surely countless stories to be told from these sources, these five are the ones that have not been told and fit into a greater story about Alvarez's identity as an American atomic physicist.

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[34] Kevles, *The Physicists*, p. *xliii*.

Ultimately, history is storytelling and here are five stories that end up with the same conclusion: that Alvarez was at the forefront of the identity politics of American atomic physics.

Chapters two and three are closely related, as they are both about Alvarez as a writer. Alvarez's autobiography was published in 1987, but he began working on it in the early 1950s and dictated a long memoir in 1972 that came in at over a thousand pages. While it may seem natural for a well-known physicist to write an autobiography today, it turns out that physicist autobiographies are a fairly recent development. Of course, there are two major exceptions—Robert Millikan wrote an autobiography in 1950 and Arthur Holly Compton wrote one in 1956. Those two Nobel Laureates in physics were a generation senior to Alvarez; Compton was Alvarez's doctoral advisor. Alvarez considered Millikan's an exception and he thought Compton's was a book about his work, not about the man. While many of the participants in the Manhattan Project would go on to write memoirs limited to their time at Los Alamos, Alvarez decided to write a proper autobiography that covered his work in physics, his work outside of physics, and some of his personal life. The Sloan Foundation eventually hired the Pulitzer Prize winning author of *The Making of the Atomic Bomb*, Richard Rhodes, to edit the long draft into *Alvarez: Adventures of a Physicist*. This autobiography, the long draft in the Luis W. Alvarez Papers, and some correspondence allow us to understand what motivated Alvarez to write an autobiography and how that autobiography fit in Alvarez's sense of history.

Chapter three describes a physics textbook draft that Alvarez began, and then abandoned in 1952. This was at a point where Alvarez was established and very productive, but before he won the Nobel Prize in 1968. Historian and philosopher of science Thomas Kuhn notes that extremely successful scientists in established fields rarely write introductory textbooks, since the place of a textbook is at the tail end of the process of discovery to dissemination: "The scientist who writes one is more likely to find his professional reputation impaired than enhanced."[35] Textbooks are how new physicists are brought into the fold of professional physics; they are, in a sense, recruitment material. As we shall see, people often take ideas they encountered in their textbooks as absolute truth, and a myth can become, in the minds of those brought up on that textbook, a fact. In the case of physics textbooks, Alvarez was an early proponent of using the history of physics to teach the concepts in physics. To make sense of the context of Alvarez's textbook, I look at the history of physics textbooks in the U. S. and find a transition that started as early as 1940 that became the dominant style by the 1960s. Physics textbooks went from describing experimental equipment to describing nature; they went from being an ahistorical list of facts to an historical approach; they went from naming physicists only to explain the equation's name to developing heroes. What better way to develop an identity for physicists than creating heroes drawn from the history of physics and developing a mythology around them? Alvarez's textbook would have been a part of that transition had it been published, but my point is that Alvarez was ahead of

[35] Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: The University of Chicago Press, 1996), p. 20. It is worth noting that established physicists do sometimes write advanced textbooks for graduate students without loss of prestige, but Kuhn's point about textbooks being at the end of a process is relevant here.

his time in his way of thinking about physics. He was not influential enough in 1952 to have made any real waves, but he was on the right track.

In order to define an identity, it can be useful to define the boundaries to that identity. Chapters four and five help define that boundary: Alvarez played the role of useful outsider in several fields outside physics, and he also kept a "Nutfile" of letters from cranks. In 1980, Alvarez, his son, and two nuclear chemists at Berkeley published a theory that an asteroid killed the dinosaurs. Although it is largely accepted today, paleontologists initially rejected it as nutty because it posited an extraterrestrial answer to a question in an earth science. Further, the theory contradicted a firmly held preference for gradual processes in geology and evolution. Alvarez applied his physics expertise to several other fields with varying degrees of success, so I explore his experiences as an outsider to explore the differences between insiders and outsiders in one more way to explore physics identity.

Demarcation is the name philosophers of science give to the issue of what is and what is not science. In Alvarez's Nutfile, which is roughly twelve hundred pages of letters and essays and some of Alvarez's replies, we have hundreds of examples of clear non-science. When I encountered them, I initially dismissed them as useless, but I kept coming back to box 38 of the Alvarez Papers that houses them. I was interested in using unusual sources in the style of Robert Darnton's *The Great Cat Massacre*, in which he argues that obscure sources are where the historian must dig to understand a foreign culture.[36] The letters in Alvarez's Nutfile are certainly foreign to mainstream physics. I take a shot at the contentious demarcation debate, using these letters as empirical examples of non-science. As I researched and wrote, I realized that Alvarez might have appeared like a crank to scientists in other fields.

Luis Alvarez was a white man with a Hispanic name. His paternal grandfather was from Spain, but Luis W. Alvarez was "a tall, ruddy blond."[37] Biographers often tried to group him into "Latinos in Science" collections, but he resisted because he did not consider himself a minority. The identity politics of Hispanics is complex, but he argued that if a single grandparent could make him Hispanic, then why did his other grandparents not make him Irish or German or Danish? Furthermore, he was not disadvantaged and did not speak Spanish. His father was a nationally known doctor and medical columnist whose connections to the Mayo Clinic helped Luis get his job at Berkeley. He was in no way at all a Chicano. The Alvarez Papers are full of clues to his identity and the desire in the Chicano community for role models like Alvarez. Despite his objections, he sometimes ended up on lists of the most influential Latinos. Here we take a different type of identity—ethnic and racial identity—and contrast it with his chosen identity as a physicist.

Beginning with the simple premise that Alvarez should be studied more carefully, a story developed about the identity of American atomic physicists. It is natural that such a rich source should prove fertile ground for a number of stories about American physics,

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[36] Robert Darnton, *The Great Cat Massacre and Other Episodes in French Cultural History* (New York: Vintage Books, 1984), p. 5.
[37] Alvarez, *Adventures*, p. 9.

but admittedly, mine is an unusual collection. It is perhaps closest to the microhistory most often associated with Carlo Ginzburg's *The Cheese and the Worms*. He used a sixteenth century miller and his theory on the origins of life to explore Italian cultural history. Questions of representation arise—what can someone say about a culture while discussing a single individual? This is a legitimate question, but it is difficult to ignore sources simply because they are challenging. Perhaps Ginzburg's Menocchio was not the average or representative Italian miller in the sixteenth century, but he was there, he experienced it, and his two trials for heresy left a written record. I have it a little easier than Ginzburg in that—besides having more abundant documentation—I am not claiming to capture an entire culture, just to witness the seeds of one particular strain of thought in American physics. Ginzburg asks "if what emerges from Menocchio's speeches is not a 'mentality' rather than a 'culture.' Appearances to the contrary, this is not an idle distinction."[38] I would be perfectly happy to capture a mentality, namely, the beginnings of an identity.

Another distinction between this work and Ginzburg's is that his is bottom-up history and mine is definitely top-down. I have collected five stories about one of the most prominent and influential physicists of twentieth century America. Although the Nutfile chapter addresses outsiders and nuts, these stories still deal with Alvarez's perspective. Perhaps more research would allow other stories to emerge. I chanced into a meeting with one of Alvarez's Spiral Reader operators. These machines for processing

[38] Carlo Ginzburg, *The Cheese and the Worms: The Cosmos of a Sixteenth-Century Miller* (Middlesex, England: Penguin Books, 1982), p. *xxiii*.

the massive output of images produced by the hydrogen bubble chambers required workers to enter data points. One of those workers now runs the Robot Repair Shop in a flea market in Sacramento. His experiences entering data would certainly put the day-to-day operation of producing data on more concrete terms. Alvarez and the leaders of the team that helped earn Alvarez a Nobel Prize have passed on, but this curious character still has stories to tell. Unfortunately, I met him too late to embark on a new chapter.

It is clear that Alvarez is a worthy topic for the historian of science. As Galison notes, "we are not going to find even one other physicist 'like' Alvarez—no one else flew in the chase plane over Nagasaki, created the largest bubble chamber group in the world, and eventually withdrew from particle physics because he found the routine unbearable."[39] This may again raise the issue of representation, but Galison's point is that no one physicist is perfectly representative, but Alvarez was certainly interesting. What can a person say about identity without noting that there is no idealized example of a physicist, a Chicano, or an insider? We can reject the notion of an idealized American physicist and instead study the real examples the archives give us.

[39] Galison, *Image and Logic*, p. 61.

Chapter 2: *Alvarez on Alvarez: The Autobiography*

> The probability that I would be happy with the work of a biographer is rather low; I haven't been happy with Ernest Lawrence's official biography, to a large extent because it was written by someone who never met his subject.[1]
>
> - Luis Alvarez on the state of physics autobiography

On August 6, 1945, Luis Alvarez spent the long hours flying back from the raid on Hiroshima writing a letter to his four-year old son Walt.[2] His plane had followed the *Enola Gay* on its successful mission to destroy an entire city. The mission did not depend on Alvarez to succeed; his mission was just to observe the blast and try to determine its power. Although he had been instrumental in devising the detonation system of the "Fat Man" bomb dropped over Nagasaki, that achievement had little to do with Alvarez's mission over Japan. Once there, he wanted to record for his son his thoughts: "What regrets I have about being a party to killing and maiming thousands of Japanese civilians this morning are tempered with the hope that this terrible weapon we have created may bring the countries of the world together and prevent further wars."[3] The reader may wonder why that is of interest—everybody there must have had a sense of their place in history. Los Alamos head Robert Oppenheimer famously quoted the

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[1] Alvarez autobiography draft dated 14 May 1972, LWAP box 46, folder "Autobiography p. 1-100," p. 1.
[2] Alvarez, *Alvarez: Adventures of a Physicist* (New York: Basic Books, 1987), p. 8. The prologue, "The Hiroshima Mission," details that flight. The letter is printed in its entirety in Peter Trower, *Discovering Alvarez: Selected Works of Luis W. Alvarez with Commentary by His Students and Colleagues* (Chicago: University of Chicago Press, 1987), pp. 69-70.
[3] Alvarez, *Adventures*, p. 8.

Bhagavad-Gita—"Now I am become Death, the destroyer of worlds"[4]—to describe his role in making the atomic bomb. His prepared words suggest he knew his significance.

Alvarez wanted more than to help design the implosion method used at Trinity and Nagasaki. He wanted to witness both the test in the New Mexico desert and the Hiroshima blast. He thought of himself as an adventurer. Biographer Richard Rhodes described Alvarez's efforts to witness history:

> That's how he came to fly the back up mission over Hiroshima. He wanted to get there and thought, "How am I going to do this?" and then he realized that the bomb we were using on Hiroshima had never been tested at full yield. … So he convinced Oppenheimer someone needed to measure the yield. Then he invented the instrument to do that.[5]

Alvarez told Rhodes that "it wasn't that he thought it was really important to test the yield of the bomb. It was that he wanted to be there."[6] Rhodes added that Alvarez "wanted to be there, as he said more than once to me, when things happened."[7] He convinced Oppenheimer to let him operate his equipment and once on the plane, he wrote the well-publicized letter to Walt.

Alvarez described the mission a bit differently in his autobiography. "We were confident it would work, and Robert [Oppenheimer] wanted to know its explosive yield. Hence my assignment."[8] In this version, Oppenheimer was the initiator, but perhaps it is simply good form not to admit publicly that such a dangerous mission was concocted so

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[4] Richard Rhodes, *The Making of the Atomic Bomb* (New York: Simon & Schuster, Inc., 1986), p. 676.
[5] Richard Rhodes interview by author, 20 May 2009, p. 16. "Biographer" may not be the right term. For more on Rhodes's relationship with Alvarez, see below in the section on Alvarez's autobiography.
[6] *ibid*., p. 44.
[7] *ibid*.
[8] Alvarez, *Adventures*, p. 4.

that a thirty-four year old adventurer could witness the destruction of a city. However, Alvarez did express his role as that of an adventurer:

> In July 1943 I had attended the briefing for the largest conventional bombing raid in history, the bombing of Hamburg by the British Royal Air Force. Now I was being briefed for an equivalent raid by a single plane. Then I had been a spectator; now I was a participant.[9]

Alvarez collected adventures. Rhodes described him as "someone who goes out there and makes new discoveries, and collects places where he has been."[10] Alvarez introduced a story in his autobiography thus: "Since this book is highlighting adventures," as if his memoirs were meant to be a collection of adventures.[11] Alvarez bragged: "I've walked on eight continents, visited 55 of the 157 United Nations countries, and seen 27 more. New Guinea is the largest landmass I've not yet walked on."[12] He was checking countries and continents off a list. When Alvarez met Rhodes's wife, Ginger, Rhodes recalled that

> He first asked her, "is that your real name or is it a nickname?"
>
> "No it's my real name."
>
> "Ah well, you're my third Ginger: Ginger Rogers" and then he had somebody else.[13]

Witnessing the bombing of Hiroshima was another adventure to check off a list and he wanted a record of it, from high-speed photography of the blast to a letter to his son.

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[9] *ibid.*, p. 6.
[10] Rhodes interview by author, 20 May 2009, p. 24.
[11] Alvarez, *Adventures*, p. 269.
[12] *ibid.*, p. 271.
[13] Rhodes interview by author, 20 May 2009, p. 24.

It seems obvious now that such a momentous event should be documented, that future generations would need to understand what Alvarez and his colleagues thought. However, nearly all the men who worked on the bomb waited for the news back in the continental United States. Alvarez wanted to be there and to document it for his son. Alvarez, then thirty-four and not yet an established leader, showed an early interest in his place in history.

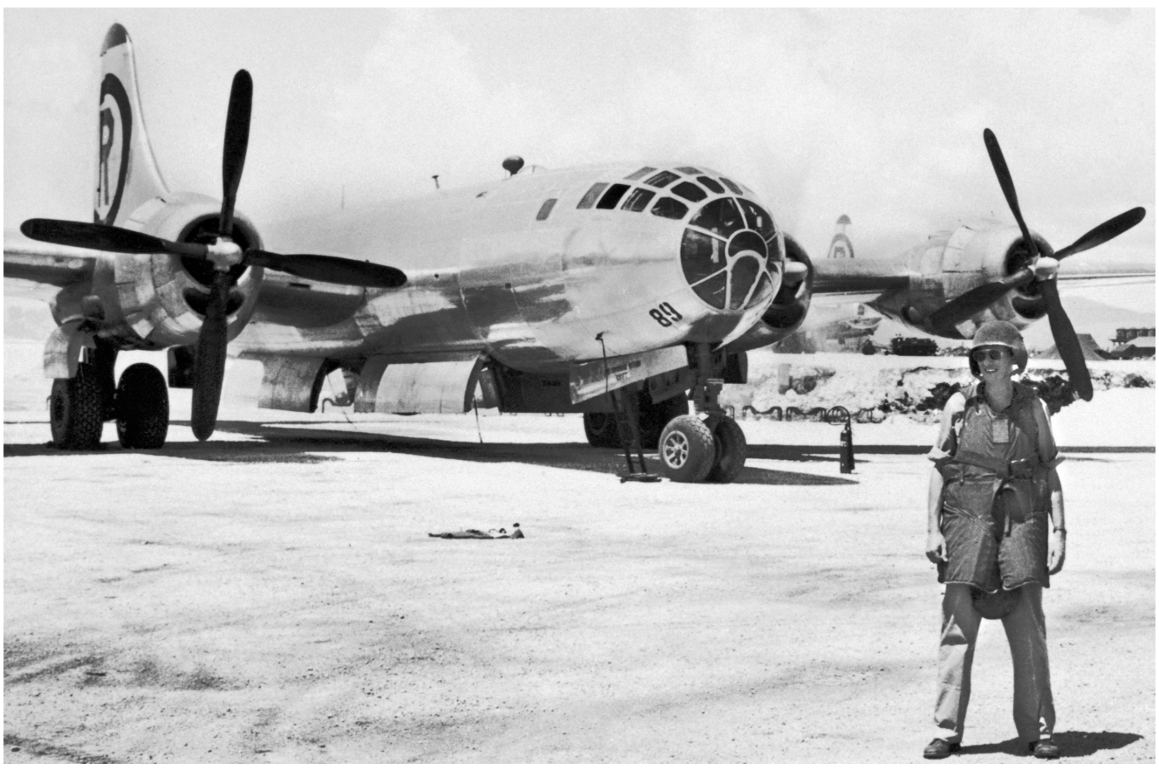

Figure 2.1: Alvarez in a flak jacket standing in front of *The Great Artiste* on the island of Tinian preparing to observe the bombing of Hiroshima in 1945.

ALVAREZ WANTED TO BE REMEMBERED

I never met Luis Alvarez, but this is not a biography. However, it is appropriate that the author of a work such as this acknowledge Alvarez the man, especially since he clearly wanted to be remembered. History is essentially storytelling, yet my protagonist remains mysterious. Alvarez had a rich and varied life outside atomic physics. Some of his life and personality fed into his career; other parts distracted from it. Ultimately, he withdrew from hydrogen bubble chambers out of frustration with the field and explored other avenues for his creative energy. Some of those projects were described in chapter four, but many others were not. He was a pilot, an inventor, a businessman, an administrator, a teacher, and a family man. I cannot capture his persona here, but, given his clear interest in telling his personal story, it would be disrespectful to ignore the man entirely. I am using Alvarez, but I hope that he would appreciate my thesis about his role in the developing identity of the American atomic physicist. Further, Alvarez's withdrawal from atomic physics captures another chapter in the history of American physics—after decades of dominance as the premier science, the 1970s saw physics give way to molecular biology. "Physics envy" is as real as ever as countless disciplines desire the respectability and successes of physics, but money and graduate students have flocked to projects such as the Human Genome Project and research in protein folding. Alvarez's later career reflects that transition.

How well can we get to know Alvarez from his papers? Many facts have eluded me and many nuances are lost. Alvarez's family life is largely absent from the collection. Obviously, telephone calls and face-to-face conversations were not saved. However, with such a large collection from a packrat, a surprising level of detail emerges from his personal life. Notes from his secretary and correspondence with friends can be revealing. His autobiography draft, which is a transcript of his many hours of dictation, is full of tangents that can often become personal. To give one example, Alvarez described in some detail his troubles with phlebitis, an inflammation of the leg vein associated with a blood clot due to lying in bed too long after gal bladder surgery. He dictated in 1972 that he lived "a delicate balance between taking to little [blood thinner], which means another attack of phlebitis, and too much of the drug, which means that the important ability of blood to clot might be lessened to the point that I would have massive internal bleeding."[14] He thought that it was a big enough part of his life that he wanted to include it in his autobiography: "Since I've been living with phlebitis for the past 34 years, it deserves more than a passing notice."[15] Phlebitis made it "very painful for me to stand up for long periods of time," affecting his ability to play golf and gave him a preference for "the Chinese equivalent of a cocktail party; before going in to eat at a Chinese banquet, everybody sits down and drinks tea and talks with his neighbors" instead of standing at a Western cocktail party. It prevented him from travelling to Australia in the mid 1950s and China in 1973 and it required him to "wear heavy elastic stockings on both legs almost to my knees." He thought his phlebitis affected his lifestyle, yet he said that he included it in the draft of his autobiography "for the benefit of those who feel they know me quite well, but are quite unaware that I have this problem." The published

[14] Alvarez autobiography draft, ca. 1975, LWAP carton 3, folder "Misc. Autobiography Draft," p. 723-4.
[15] *ibid*, p. 724.

version mentions phlebitis in three sentences, but only to set up a story about how he discovered a new method of finding gallstones while resting in the hospital.[16] In a very real sense, the archives let us know things about Alvarez that his friends did not. However, the historian should not fool himself and pretend to know his subject better than those who were there. What were his musical tastes? I know he played piano, but do not know what he liked to play. How was he on the golf course? I know he played golf and was good at billiards, but those are vague descriptions. I have watched clips of Alvarez on Youtube.com, but I cannot claim to know his mannerisms or personality.

In 2003, I gave a talk at the History of Science Society on the mythology of the plum pudding model of the atom. A physicist in the audience asked why he should care that the story told in physics textbooks is a myth if it is useful pedagogically. A historian reflexively strives for accuracy, but Alvarez makes the case that a physicist needs to know how physicists actually worked, not a cleaned-up version presented in textbooks. I believe the historical record provides plenty of good pedagogy without resorting to mythology. Alvarez wanted to be known and he thought that knowing as much about his predecessors would make him a better physicist.

PHYSICS AND AUTOBIOGRAPHY

One might think that autobiographies are a genre of literature as entrenched as Augustine's *Confessions* of the late fourth century, but the idea of writing about one's life is a relatively new development. A handful of ancient examples exist, but they are fairly rare and largely disappeared after the fall of Rome not to begin their return until the

[16] Alvarez, *Adventures*, p. 94.

development of the Renaissance concept of the self. Paul Delany notes that the fact that few Roman autobiographies survived suggests that they were held in low esteem.[17] It is surprising how slow American atomic physicists were to begin writing their autobiographies; they changed the world with the quantum mechanics, the atomic bomb, and big science. Physicists and others who were at Los Alamos during the development of the atomic bomb are notable as a group for their desire to get their story in print, but full biographies emerged slowly. The study of autobiographies is also relatively new, as sociologist Diane Bjorklund noted: "In sociology, it was not until the late 1970s, as part of the increased concern with how people give meaning to the world, that there has been significant renewed interest in personal documents and in the act of reporting and writing about oneself."[18] She summarized the value of autobiography:

> Autobiographies … are a good resource for investigating changing ideas about the self. They are historical records that have been steadily produced and preserved over a lengthy time period. Autobiographers show us how people have made sense of their lives and experiences as they answered the question, "Who am I?"

Alvarez, in the subtitle of his autobiography, was a physicist. That was his identity, and it developed as an identity for a community later than one might think.

Paul Delany studied the emergence of autobiographies in England and found very few in the sixteenth century and only two hundred in the seventeenth. He found that "For the educated Englishman of that time, a recognized literary genre entitled 'autobiography' did not exist, any more than the word itself (which seems to have been

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[17] Paul Delany, *British Autobiography in the 17th Century* (London: Routledge & Kegan Paul, 1969), p. 110.

[18] Diane Bjorklund, *Interpreting the Self: Two Hundred Years of American Autobiography* (Chicago: The University of Chicago Press, 1998), p. 9.

coined by Southey in 1809)."[19] So autobiography in English literature developed gradually, presumably in England before the United States. Further, Delany argued that the rise of autobiography in England reflected the "unprecedented general social mobility" of seventeenth-century England.[20] After all, early writing about the self was about one's relationship to God, as Augustine's *Confessions* might attest. The idea of writing an entire book about one's experiences may have seemed egotistical, but tying it to God was easier.[21] Perhaps the American painter Chester Harding would have agreed with that assessment—he titled his *My Egotistography*.[22] Delany argued that this led to "the spur to increased self-awareness among seventeenth-century Englishmen."[23] Self-awareness is exactly what we are looking for in the development of the American atomic physics autobiography. As spirituality gave way to the achievements of the individual, English writers began distinguishing themselves "by extensive travels, by having taken part in important events (especially those of the Civil War, Commonwealth, and Restoration), by having discovered new scientific facts or held high offices, or by achievements of many other kinds."[24] While that sounds a lot like Alvarez with World War II substituted for those seventeenth-century wars, we should be careful not to overextend Delany's conclusions, instead acknowledging that English-language autobiography developed largely in the century before Alvarez was born.

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[19] Delany, *British Autobiography*, p. 1.
[20] *ibid*., p. 19.
[21] *ibid*., p. 107.
[22] Chester Harding, *My Egotistography* (Cambridge, Mass.: John Wilson and Son, 1866).
[23] Delany, *British Autobiography*, p. 19.
[24] *ibid*., p. 109.

The Second World War and the Manhattan Project specifically did create a small genre of memoirs by those who were at Los Alamos when the atomic bomb was being developed. At least two-dozen memoirs have been published on this experience. However, they did not come into print immediately after the war ended when the enthusiasm of victory was fresh. The first couple of books were published in the mid-1960s by Edward Teller and Eugene Wigner, but they were collections of essays on the experience, hardly qualifying as autobiographies.[25] The first autobiographies by Manhattan Project alumni were those of Otto Frisch and Leona Marshall Libby (then named Leona Woods) in 1979. Libby was the youngest and only woman to work on the first pile, an early term for nuclear reactor, at the University of Chicago. Although she was not at Los Alamos, she certainly makes an interesting scientist. Alvarez had already completed a full draft of his autobiography by 1972, so he was arguably still one of the first physicists to decide that his story needed to be told, but it is impossible to prove that some of the others did not also sit on a draft for as long as Alvarez did. Of course, General Leslie Groves, the chief military official in the Manhattan Project, published an autobiography appropriately titled *Now It Can Be Told* in 1962, but it is safe to say that he was a high profile government official who also oversaw the construction of the Pentagon building. I say those who were there, because not all of the memoirs are by physicists working on the bomb. Two wives of Los Alamos physicists wrote memoirs of

[25] Edward Teller, *The Reluctant Revolutionary* (Columbia: The University of Missouri Press, 1964) and Eugene Wigner, *Symmetries and Reflections: Scientific Essays of Eugene Wigner* (Bloomington: Indiana University Press, 1967).

their time there as well, Eleanor Jette and Phyllis K. Fisher.[26] Jette published her memoir in 1977, so she beat the physicists to the punch. The trickle of memoirs continued until the 1990s saw a torrent. The latest appears to be McAllister Hull's 2005 memoir.[27] Hull, born in 1923, will likely have written one of the last of this small genre of memoirs. However, since most are focused on just a few years at Los Alamos, the bulk of these books can hardly be called autobiographies.

Given the postwar interest in all things atomic, why did the American physicist autobiography take so long to become commonplace? Historians Michael Shortland and Richard Yeo explain that a tradition in science had developed as early as 1767 with English natural philosopher Joseph Priestley, who argued that intellectual progress in science "was not solely a product of individual genius." Instead, Priestley deflated "the role of genius—even that of Newton—and championed the role of factors such as correct method and the accumulation and communication of data."[28] That is, science is a method and body of knowledge, not a collection of men. Science is supposed to be impartial and detached.

Alvarez would disagree. In the foreword to the first issue of a short-lived physics journal edited by Bogdan Maglich, Alvarez thought physics writing was too impersonal: "Personal pronouns and personal thoughts are scrupulously exorcised; if a restaurant

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[26] Eleanor Jette, *Inside Box 1663* (Los Alamos: Los Alamos Historical Society, 1977) and Phyllis K. Fisher, *Los Alamos Experience* (Tokyo: Japan Publications, Inc., 1985).
[27] McAllister Hull, *Rider of the Pale Horse: A Memoir of Los Alamos and Beyond* (Albuquerque: University of New Mexico Press, 2005).
[28] Michael Shortland and Richard Yeo, "Introduction," in Shortland and Yeo, eds., *Telling Lives in Science: Essays on Scientific Biography* (Cambridge: Cambridge University Press, 1996), p. 17. The wording is theirs, not Priestley's.

owner similarly concentrated all his attention on dietary content, and left out all the flavoring he would quickly go broke."[29] It was not just a matter of making physics interesting, it was a matter of communicating the methods of physics—how physicists actually work, not some idealized scientific method—to the next generation. He thought physicists learn by their mistakes as much as by their successes. His graduate student Richard Muller recalled Alvarez congratulating him after breaking a very expensive piece of equipment for the first time.[30] He lamented that physics journals describing only successes and not failures: "Physicists can, and usually do hide their most serious 'goofs', so students of Physics can't learn from them as military and government officials learn from the cold facts of history."[31] After all, Alvarez strongly believed in teaching the process of discovery, not just the results. In an introduction to a former student's book, he argued that

> I have long felt that most books about science, indeed even most scientific autobiographies, neglect to mention what I would most like to know—how scientists happened to be working on their most important problems and what the nature of their thought processes was as they attacked those problems.[32]

Maglich heartily agreed on the need for history and personalizing physics, and one suspects that Alvarez had a lot to do with the publication of this historically-minded journal given the frequency of his articles and editorials. Maglich argued that the "Now Generation" had rejected physicists as "impersonal" and "dehumanized": "There is the

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[29] Alvarez, "Foreword," August 1971, *Adventures in Experimental Physics*, α (1972), p. *ii*. Maglich chose to label the issues of his journal in lower case Greek letters instead of Arabic numerals.
[30] Richard Muller, *Nemesis the Death Star: The Story of a Scientific Revolution* (New York: Weidenfeld & Nicolson, 1988), p. 24.
[31] Alvarez, "Foreword," p. *iii*.
[32] Alvarez, "Introduction," in Muller, *Nemesis*, p. *xi*.

dehumanizing myth that physical sciences are 'conducted' from a podium chaired by 'well-informed' committees, guided by infallible theories; that all runs with computerized efficiency."[33] Admittedly, Alvarez came close to making that a reality in his quest for bigger, more efficiently run systems involving large teams and many non-physicist machine operators. When that happened, he left particle physics, as we shall see.

ALVAREZ DECIDES TO WRITE AN AUTOBIOGRAPHY

Luis Alvarez's mentor died at the age of 57. Ernest Lawrence passed away unexpectedly in 1958 after a bout of colitis. Alvarez worried about his legacy were he to have a similar fate. Particularly important to him was his family:

> I realized in 1969 that my two youngest children, then three and five years old, would know little of me if I were to die suddenly, at my then age of fifty-seven, as my friend and mentor Ernest Lawrence had. So for them I began dictating my life story, with the strong conviction that I would never take the time to prepare it for publication.[34]

By early 1972, Alvarez reported that

> several of my young physics friends have said, after a luncheon table discussion during which Emilio Segrè and I swapped reminiscences of our lives in physics, "Luie, I hope that some day someone writes your biography—you've had so many fascinating experiences."[35]

Alvarez settled on a format that would allow him to record his memories but not take too much of his time. He would dictate his memoirs into a tape recorder to be transcribed by his secretary. This technique seemed to him to balance the effort of writing with the available time he could commit to such a project.

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[33] Bogdan Maglich, "Editorial," *Adventures in Experimental Physics*, α (1972), p. 187.
[34] Alvarez, *Adventures*, p. xi.
[35] Alvarez autobiography draft dated 14 May 1972, p. 1. LWAP box 46, folder "Autobiography p. 1-100."

> I have dictated almost all of my personal correspondence for many years, and all of that practice has reduced the editing tasks to manageable proportions. So I'll try this technique for a few chapters, and if my young friends find the results interesting, I'll continue; if not, this introduction will join in oblivion the first several chapters of a text book on nuclear physics that I wrote in the early 1950's.[36]

The dictation method resulted in two drafts, one started around 14 May 1972, and another started in the early 1980s. That method does have its faults. Rosa Segrè, the wife of physicist Emilio Segrè, asked Alvarez about his methodology:

> I found myself sitting next to Luis Alvarez at a dinner party, and he told me that he was writing his memoirs. "Do you spend so much time going through your filing cabinets reading old letters?" I asked him. "Oh, no," he said; "I just write down what I remember."[37]

She added that "Emilio researched tirelessly in his obsession with accuracy and truth." That certainly raises questions about the accuracy of Alvarez's autobiography and the archival draft, but the argument here is about Alvarez's impression of the need for more history in physics.

In 1962, Alvarez gave the Berkeley physics department Faculty Lecture titled "Adventures in Nuclear Physics." This prestigious lecture generally summed up the research performed by the lecturer, but Alvarez followed a recent trend in a casual style of lecturing that allowed him to historicize his work more than the traditional style would allow.

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[36] *ibid.*, p. 2. That textbook is the subject of my chapter three.

[37] Rosa Segrè, "A Word From Rosa," in Emilio Segrè, *A Mind Always In Motion: the Autobiography of Emilio Segrè* (Berkeley: University of California Press, 1993), p. 297.

> A faculty research lecture is different from the usual impersonal scientific talk, where one says, "The apparatus was designed and built, measurements were taken, and the following results were obtained."[38]

Alvarez meant to take a more casual tone, "and use the first person without further apology."[39] He explained that "The word 'Adventures' in the title of this talk is intended to convey the feeling that most physicists have about their work: To us, the pursuit of science <u>is</u> an adventure."[40] Indeed, Alvarez's career "has led me into adventures of the more familiar kind—trips to foreign lands in search of cosmic rays, to a previously secret Russian laboratory, to test flights in new aeroplanes, and to uncomfortably close proximity to exploding atomic bombs."[41] He did not take the titular concept of "adventures" lightly; his autobiography would be subtitled *The Adventures of a Physicist* in light of the fact that he viewed his career as an adventure.

The autobiography was ostensibly meant for his children but Alvarez initially showed it only to his closest student, Richard Muller.[42] At least until 1979, he had shown it only to Muller and his son Walter some time after Walt joined the geology department at Berkeley in 1977.[43] Alvarez added, "For many years, only three people knew that I was writing my memoirs—my wife, Jan, was the third."[44] Apparently, his secretary transcribing the tape did not count. However, the historical significance of his life and work was never far from his mind. His friends and students pushed him to get an

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[38] Alvarez, Faculty Lecture, "Adventures in Nuclear Physics," University of California, Lawrence Radiation Laboratory, UCRL-10476 (Mar. 1962), p. 1.
[39] *ibid*.
[40] *ibid*., p. 2. Emphasis in original.
[41] *ibid*.
[42] Alvarez, *Adventures*, p. *xi*.
[43] Alvarez to Robert S. Shankland, 24 Sep. 1979, LWAP box 40, folder "S [1 of 3]."
[44] Alvarez, *Adventures*, p. *xi*.

autobiography in print. Robert S. Shankland, a physicist and long-time friend from Alvarez's graduate school days at Chicago,[45] prodded him to publish, arguing that "Your Autobiography is not needed now for your own reputation. Its great value will be for the Historical Record."[46] He listed Alvarez's accomplishments, ending with "Your style of writing is exceptional."[47] Shankland went on to argue that "You should write it by all means, but do not publish it too soon"[48] because he thought Alvarez still had a lot of adventures ahead. Alvarez agreed, saying that he would wait to publish it after his death.[49] The autobiography was eventually published in 1987, a year before Alvarez's death. It would seem that his students' constant push to publish won out.

PETER TROWER PUSHES FOR PUBLICATION

A former student of Alvarez's, Peter Trower, became an impetus for publication. Around 1982, he proposed a festschrift for Alvarez and began shopping around for commentators and a publisher. Alvarez's papers have letters at least back to February of 1982 between Trower and publishers that were carbon copied to Alvarez. The festschrift included twenty-one articles by Alvarez, along with commentary by people who were witness to those experiments or could judge the importance of each article. *Discovering Alvarez: Selected Works of Luis W. Alvarez With Commentary by His Students and Colleagues* was eventually published by the University of Chicago Press after Trower

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[45] Alvarez to Robert Shankland, 3 Mar. 1978, LWAP box 40, folder "S 1978-87 [2 of 3]," p. 2. Alvarez says they have been friends since about 1931.
[46] Shankland to Alvarez, 14 Sep. 1978, LWAP box 40, folder "S [1 of 3]." Emphasis in original.
[47] *ibid*. Emphasis in original.
[48] *ibid*. Emphasis in original.
[49] Alvarez to Shankland, 24 Sep. 1979, LWAP box 40, folder "S [1 of 3]."

was turned down by such presses as the University of California Press[50] and the Alfred P. Sloan Foundation,[51] which would eventually publish the autobiography.

Peter Trower found out about the autobiography draft some time in the early 1980s and started pushing to get it published. He also volunteered to edit it, despite the distance involved. Trower, a professor of physics at Virginia Polytechnic Institute and State College in Blacksburg, Virgina, had to deal with editing by mail. It was Trower's idea to adapt the "adventures" theme into the title. A short-lived physics journal edited by Bogdan Maglich started in 1972 provided further inspiration, as Alvarez recalled: "Bogdan borrowed the title of this very valuable publication from that of the 'Faculty Research Lecture' I gave in 1962—'Adventures in Nuclear Physics,' and that my editor, Peter Trower, has revived for this book."[52] Despite the help, Trower was perhaps not an expert editor. Alvarez's draft copies in his papers are all in the range of one thousand pages and not ready for widespread publication. Alvarez noted that his dictation technique would be an experiment because he thought

> that the spoken and the written word are really two quite different dialects of the English language. My two recorded lectures would have sounded very stilted had I read them from the text that I later wrote, and conversely, verbatim transcripts of the lectures the way I actually gave them would have sounded ungrammatical and perhaps even illiterate, even though I knew they were good lectures.[53]

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[50] James Clark, President, University of California Press to Trower, 30 Apr. 1984, LWAP box 40, folder "Peter Trower." Clark pointed out that the commentary by Muller was good, but those by Segrè and Pitzer were "weak."

[51] Eric Wanner, Sloan Foundation to Trower, 28 Oct. 1983. LWAP box 40, folder "Peter Trower."

[52] Alvarez, autobiography draft dated 17 Aug. 1984, page 1 of "X-raying an Egyptian Pyramid." This page, along with a page 11 and a page 22, are out of sequence. LWAP carton 3, folder 16, "Misc. Autobiographical Draft 5."

[53] Alvarez autobiography draft dated 14 May 1972, p. 2. LWAP box 46, folder "Autobiography p. 1-100."

He was right—the drafts in the Luis W. Alvarez papers are long and rambling with many long tangents. He needed a professional writer.

The man who would eventually turn those thousand page drafts into the finished book was Richard Rhodes. Rhodes is best known for his 1986 Pulitzer Prize winning book, *The Making of the Atomic Bomb* that tells the story of the Manhattan Project in excellent detail. Alvarez called *Making* "A great book. Mr. Rhodes has done a beautiful job, and I don't see how anyone can ever top it."[54] Alvarez and Rhodes first interacted when Rhodes' publisher sent advance copies out for the type of endorsements above. Alvarez and his Berkeley colleague Emilio Segrè both responded, as Rhodes recalled, "Rhodes got some of the science wrong. Can we fix it?"[55] Alvarez and Rhodes met in 1982 to talk over Rhodes' *Making*, which was then titled *Ultimate Powers*.[56] They hit it off and would get a chance to work together again when the Sloan Foundation hired Rhodes to rework Alvarez's drafts into a publishable work. The Sloan Foundation had funded *Making* and had begun a series of scientist autobiographies. Here our story shifts from a physicist wanting to publish an autobiography to a science education foundation trying to find scientists who would be willing to do just that.

THE SLOAN FOUNDATION SERIES

In March 1975, Art Singer of the Sloan Foundation read a review by Victor McElheny of *The Path to the Double Helix* by Robert Olby in the *New York Times Book*

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[54] Richard Rhodes, *The Making of the Atomic Bomb* (New York: Simon & Schuster, Inc. 1986), "Praise for *Making of the Atomic Bomb*."
[55] Rhodes interview by author, 20 May 2009, p.22.
[56] Richard Rhodes to Alvarez 6 May 1982, LWAP box 39, folder "R 1978-87."

Review. Singer replied to my inquiry into why the Sloan Foundation published a series of scientist autobiographies:

> The review contained two sentences that struck me: 1) "James Watson's book The Double Helix was perhaps the first literate, popular memoir in the history of science." 2) "The evidence of how a crucial scientific discovery is made cannot be recovered from its formal exposition in scientific papers."
>
> These observations triggered in my mind the need for more "literate, popular memoirs," which might be brought into being by the Sloan Foundation.[57]

Singer went on to describe the process of getting the "Science Book Series" written and published. He found receptive audiences at the Sloan Foundation where Steve White joined the effort and at Princeton's Institute for Advanced Study, where 1952 physics Nobel laureate Ed Purcell agreed to join the committee, even though he did not write an autobiography. The Science Book Series, as Singer calls it, comprises seventeen books from 1979 to 1991.[58] The series includes such notable scientists, mathematicians and economists as Francis Crick, I. I. Rabi, Rita Levi-Montalcini, Freeman Dyson, and others who were all very accomplished, including eight Nobel laureates. According to Singer, "Dyson and [Lewis] Thomas were best-sellers in the U.S., [François] Jacob in France."[59] The Sloan Foundation commissioned a study of library circulation and found that "Nearly all the libraries we sampled had bought the books (in some cases more than one copy)

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[57] Draft of memoirs by Art Singer, chapter "Science Book Series." Personal correspondence from Art Singer to the author, 23 Apr. 2009, p. 58.

[58] The series is searchable in online catalogs with the tag "Sloan Foundation Series." Note that the University of Texas libraries have all seventeen, but those by Freeman Dyson and George Stigler do not come up under that search because they are not tagged the same way. Singer's memoirs draft says there were eighteen, but a search through the online catalogs of the Library of Congress, the University of Texas, Austin, and the University of California turn up only seventeen.

[59] Singer, "Science Book Series," p. 59.

and most had lively circulation."[60] Sloan Foundation Series committee member and Nobel Prize-winning economist Paul Samuelson wrote to Singer that "Being on the Sloan Science book Committee ranks as an important life experience. It was like belonging to an intellectual dinner club on a rotating geographical basis and rubbing elbows (so to speak) with some of the finest minds of our age."[61] The series of autobiographies with one biography—physicist John Rigden "worked closely with Rabi in writing"[62] the I. I. Rabi biography—was a success at making sure a generation of scientists got their story out.

Why did Singer think there was such a need? In his words,

> I think it was one of the most important things I did in my Sloan tenure. I say that because it can never be done again. There have been dozens of biographies of Einstein, but none in his own words. The scientists/writers in the Sloan Series are now mostly dead. Their books will not be revised from another perspective. They are unique.[63]

It is worth noting that Einstein did write two autobiographical sketches, but one was never translated into English and the other focuses on his physics, not his life.[64] Singer realized that "This moment (1975) might provide a unique opportunity to capture the stories of a group of historic scientists in their own words."[65] Surprisingly, there are almost no autobiographies of American physicists or even physicists living in the U.S. before Art Singer had the idea for the Sloan series. At least two physicists'

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60 *ibid*.
61 *ibid*., p. 60.
62 John S. Rigden, *Rabi: Scientist and Citizen* (New York: Basic Books, Inc., 1987), p. *xii*.
63 Singer, "Science Book Series," p. 58.
64 The un-translated one is "Erinnerungen-Souvenirs," Schweizerische Hochschulzeitung 28 Sonderheft, (1955), pp. 145-153; the un-biographical one is "Autobiographical Sketches" (1946) in Paul Arthur Schilpp, ed., *Albert Einstein: Philosopher-Scientist* (New York: Tudor Publishing Co., 1951).
65 Singer, "Science Book Series," p. 58.

autobiographies came into print between Singer's idea for a science series and the release of the first in that series,[66] but those would clearly not have influenced his idea to start the series. Interestingly, neither were particularly influential physicists.

The only real exception to the absence of autobiographies is Robert Millikan's *Autobiography* of 1950. Alvarez notes the exception in passing while discussing the lack of proper autobiographies in his own autobiographical draft of 1972:

> There haven't been many autobiographies of American physicists. Robert Millikan's [*Autobiography*] is an exception, and our scientific careers hardly overlapped. Arthur Compton's "Atomic Quest" is the personal narrative of his involvement with the Manhattan District's efforts to build nuclear reactors and atomic bombs. There is very little of Arthur Compton the person and the scientist in this excellent book, so it can hardly be called an autobiography.
>
> Seabury's biography of R.W. Wood is said by those who knew Wood to be substantially autobiographical. Seabury knew very little physics, so Wood wrote explanatory material for the book, and then apparently supplied many of the anecdotes in the form they eventually took in the book.[67]

It would seem that, in 1972 and despite Millikan's autobiography, Alvarez agreed with Singer's appraisal that there were not enough autobiographies of physicists. Chemist Erwin Chargaff called scientific autobiography a "most awkward literary genre" in 1968 precisely because they were "the account of a career, not a life."[68] Robert Millikan had been one of the first Americans to earn a Nobel Prize in physics. The editors of *Time* had declared him "the most famous and respected American scientist"[69] of his day. Historian Daniel Kevles noted that Millikan, along with "Hale, Whitney, Jewett, and Walcott" were

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66 Those two books are Walter M. Elsasser, *Memoirs of a Physicist in the Atomic Age* (New York: Science History Publications, 1978) and Philip Morse, *In at the beginnings: A Physicist's Life* (Cambridge, Mass.: MIT Press, 1977).

67 Alvarez autobiography draft dated 14 May 1972, p. 2. LWAP box 46, folder "Autobiography p. 1-100."

68 Shortland and Yeo, *Telling Lives in Science*, p. 8.

69 Daniel Kevles, *The Physicists* (Cambridge, Mass: Harvard University Press, 1971), p. 183.

“The public leadership of American science” in the 1920s.[70] Further, Alvarez related the often repeated story that

> Publicity was measured in an absolute unit, the “kan.” That unit was too large for ordinary application and a practical unit one one-thousanth that size served in its place, the “millikan.”[71]

Another version of the “possibly apocryphal story about” Millikan “had defined a new unit for the quantitative measure of conceit and had named the new unit the kan.”[72] It seems fitting that Millikan would be the first American physicist to write an autobiography, and his may be the birth cries of American physicist identity, but one that was premature and found little fertile ground among his peers, students, or the public imagination.

Of course, Alvarez may have simply missed some autobiographies. By consulting a bibliography of American autobiography, one finds four autobiographies by American physicists published between 1945 and 1972, the date of Alvarez’s draft.[73] Alvarez was aware of only one of those four books. William Weber Coblentz published an autobiography in 1951; Alvarez mentions Compton’s autobiography of 1956; Harvey Lincoln Curtis published an autobiography in 1958; and George Gamow released his in 1970. The physicist Alvarez mentions, Compton, was by far the most well known of the four before 1970; Coblentz and Curtis were not major figures in physics. Gamow would

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[70] *ibid.*, p. 170.
[71] Alvarez, *Adventures*, p. 29.
[72] Robert G. Mortimer, *Mathematics for Physical Chemistry, Third Edition* (Oxford: Elsevier Academic Press, 2005) p. 12.
[73] Mary Louise Briscoe, *American Autobiography 1945-1980: A Bibliography* (Madison: The University of Wisconsin Press, 1982). This bibliography includes an autobiography by Leopold Infeld, but he was Polish, worked in Canada, and returned to Poland. He did not work in the U.S.

later be as well known as Compton, publishing only two years before Alvarez dictated his autobiography. Even if Alvarez did miss some, part of the reason for analyzing his autobiography is that he thought there was not enough autobiographical writing by atomic physicists. That is, we are interested in Alvarez's impression of the state of physics identity and we are interested specifically in atomic physicists. Coblentz worked on infrared radiometry and spectroscopy, which are of interest to astronomers. He and Curtis worked at the National Bureau of Standards. Alvarez seems to have overlooked Gamow, the Russian-born theorist who worked on quantum mechanics. However, he can be forgiven: the bibliography I used left out Millikan's 1951 autobiography.

Even the several biographies of American physicists disappointed Alvarez. He lamented that he knew very few good biographies of his peers and heroes. He even worried that he would never get a good biography.

> The probability that I would be happy with the work of a biographer is rather low; I haven't been happy with Ernest Lawrence's official biography, to a large extent because it was written by someone who never met his subject.[74]

Alvarez noted one exception: Enrico Fermi. Alvarez devoted a chapter of his own autobiography—chapter six, "Working with Fermi"—to gush about how incredibly intelligent Fermi was. One example of Alvarez's reverence for his boss of about six months during WWII was about how Fermi had little need for books. Fermi once realized that they needed a formula that neither he nor Alvarez could remember. Alvarez

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[74] Alvarez autobiography draft dated 14 May 1972, p. 1. LWAP box 46, folder "Autobiography p. 1-100." The Lawrence biography he mentioned is Herbert Childs, *An American Genius; The Life of Ernest Orlando Lawrence* (New York: Dutton, 1968).

offered to go get a standard textbook from next door. "Enrico said not to bother; he'd derive it."[75] Alvarez was astounded by Fermi's command of physics and mathematics:

> Enrico wrote James Clerk Maxwell's classic electromagnetic field equations on the blackboard and then in about six separate steps derived the formula. The most remarkable aspect of this tour de force was that Enrico worked through his derivation line by line at a constant rate, as if he were copying it out of a book.

Alvarez was not Fermi's only fan. Laura Fermi, Enrico's wife, was one. Emilio Segrè was Fermi's student and, in Alvarez's words, "his trusted colleague and longtime associate."[76] Segrè would go on to win a Nobel prize of his own in 1959. The two were a dedicated fan club that Alvarez apparently never acquired:

> Enrico Fermi comes to life beautifully in two biographies by people who knew him and his work intimately, and loved him as well—Laura Fermi and Emilio Segrè. But since I am no Fermi, and in addition, no one knows my whole personal or scientific life as well as Fermi's biographers knew his, such fine biographies are not in the cards for me.[77]

So Singer and Alvarez agreed by the middle of the 1970s that there were not enough autobiographies of American physicists and not enough good biographies. They would both contribute to fixing that, one as an editor, one as an author.

Art Singer started the work of organizing a steering committee and getting funding from the Alfred P. Sloan Foundation. That work went well, but he realized the real problem would be the realization

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[75] Alvarez, *Adventures*, p. 117.
[76] *ibid*., p. 115.
[77] Alvarez autobiography draft dated 14 May 1972, p. 2. LWAP box 46, folder "Autobiography p. 1-100." Segrè himself would have a son who wrote about him: Claudio Segrè, *Atoms, Bombs, and Eskimo Kisses* (New York: Viking, 1995). The two books Alvarez alluded to are Laura Fermi, *Atoms in the Family: My Life With Enrico Fermi, Designer of the First Atomic Pile* (London: Allen & Unwin, 1955) and Emilio Segrè, *Enrico Fermi: Physicist* (Chicago: The University of Chicago Press, 1970).

> that not many scientists had the demonstrated capacity to write for a general audience. We would have to choose carefully. [Sloan Foundation vice-president] Steve [White] and I later set two criteria—eminence in their field and some writing in their track record beyond technical papers.[78]

Perhaps this is one of the reasons that physicists had not written many autobiographies. British physicist and novelist C.P. Snow famously noted this divide in his *The Two Cultures*. Snow, perhaps more famous for criticizing the scientific illiteracy of those in the liberal arts, pointed out the scientists' lack of literary curiosity. Most scientists, he claims, would say something like, "'Well, I've *tried* a bit of Dickens,' rather as though Dickens were an extraordinarily esoteric, tangled and dubiously rewarding writer, something like Rainer Maria Rilke."[79] Physicists can be openly hostile to social and cultural sciences. Physicist Alan Sokal published a nonsense article in a cultural studies journal, then revealed the hoax to demonstrate the lack of rigor in cultural studies.[80] Richard Feynman had his own critique of the social sciences he called "Cargo Cult Science," in which he takes a jab at disciplines that try to emulate the successes of physics by superficially embracing big equations as if that will automatically bring success.[81] Alvarez recalls a story about legendary physicist Paul Dirac's distaste for reading. When Ernest Lawrence saw Dirac off on a long train ride with the gift of two books for the journey, Dirac refused, claiming that books "interfered with thought."[82]

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[78] Singer, "Science Book Series," p. 59.
[79] C.P. Snow, *The Two Cultures* (Cambridge: Cambridge University Press, 1959), p. 12.
[80] The hoax article was Alan Sokal, "Transgressing the Boundaries: Toward a Transformative Hermeneutics of Quantum Gravity," *Social Text* 46/47 (1996), p. 217; the hoax was revealed and discussed in Sokal, "A Physicist Experiments with Cultural Studies," *Lingua Franca* (May/June 1996), p. 62.
[81] See "Cargo Cult Science" in Richard Feynman, *Surely You're Joking, Mr. Feynman!" Adventures of a Curious Character* (New York: W.W. Norton, 1985), pp. 308-317.
[82] Alvarez, *Adventures*, p. 87.

Another brilliant physicist and inventor of the quark, Murray Gell-Mann, suffered terribly in writing a for a lay audience:

> *The Quark and the Jaguar* was agony for him to write, and a considerable pain to live through for his friends and colleagues. … Gell-Mann's absorption in *The Quark and The Jaguar* reduced his mental presence at the Santa Fe Institute and at Los Alamos. Some friends say it even had something to do with the mild heart attack he suffered late in 1992.[83]

Perhaps it is a matter of physicists not being good writers. After the nuclear attack on Hiroshima, the world over knew and marveled—or recoiled—at the work of American physics, yet those physicists took until the late 1970s to start publishing autobiographies. Maybe it would take the combination that Singer sought: an accomplished physicist who could write well. Robert Oppenheimer was famously eloquent, but after his security trial ended in 1954, he was not inclined to write a memoir. Singer would need someone who fit that description and had the motivation.

Alvarez was not one of those talented writers and he knew it. Despite physicists like Robert S. Shankland's opinion on Alvarez's writing style discussed above, Art Singer disagreed: "The first manuscript we received from Alvarez was unpublishable. We then hired Dick Rhodes to work with him. That made all the difference."[84] Rhodes, mentioned above for his *The Making of the Atomic Bomb*, had worked with Alvarez for that book. Singer had been Rhodes's contact when Rhodes got some funding to finish writing *Making of the Atomic Bomb*.[85] These relationships can be difficult. The

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[83] David Berreby, "The Man Who Knows Everything; Murray Gell-Mann," *New York Times* 8 May 1994.
[84] Art Singer to author. Personal correspondence, 23 Apr. 2009.
[85] Richard Rhodes, *How to Write: Advice and Reflections* (New York: Morrow, 1995), p. 158.

relationship between Alvarez and Rhodes was certainly better than that of Murray Gell-Mann and his ghost writers:

> Gell-Mann plowed through three. One, who had helped produce the 32-page proposal that sold the book, bowed out after that point and wrote his own book ("Complexity," by Roger Lewin); the next one simply couldn't bear the flaws Gell-Mann would find in everything he wrote and dropped out; a third wisely decided his three-month job was only to edit and encourage as Gell-Mann agonized over his own hen-scratchings.[86]

Alvarez and Rhodes had only minor disagreements; Alvarez wanted to use nicknames while Rhodes wanted to use proper names, but otherwise, Rhodes reports that "He didn't resist" efforts to cut tangents and technical details and Rhodes deferred to Alvarez on technical matters.[87] A look at the drafts in the Luis W. Alvarez papers confirm that they were far too long for publication and riddled with extremely long tangents. Rhodes alternately described his work as "editing his memoir" and "organize his memoir."[88] He described the task: "he and Trower tried to turn these thousands of pages of dictation into a book; they didn't really know how."[89] "I don't think that Trower had done much more than … cut and paste to give it some kind of chronology."[90] Rhodes suspects that he came into the project "with a certain amount of unease on their part. I don't think Trower really appreciated" Sloan bringing in a professional writer, noting that he was "buried in the acknowledgments."[91] Besides editing down, Rhodes also rearranged for effect. One instructive example was beginning the story *in medias res*, or taking one of the most

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[86] Berreby, "Murray Gell-Mann."
[87] Rhodes interview by author, 20 May 2009, p. 20.
[88] Rhodes, *How to Write*, pp. 16, 51.
[89] Rhodes interview by author, 20 May 2009, p. 13.
[90] *ibid*., (p. 19)
[91] *ibid*.

exciting passages—the flight over Hiroshima—and putting it at the beginning.[92] They had some lunches and talked by phone sometimes to clear up issues, but Rhodes mostly just converted the dictation into prose. Alvarez and Rhodes worked together smoothly and within a few years, *Alvarez: Adventures of a Physicist* was published in 1987.

Rhodes suggested two reasons that physicists might not write autobiographies: it "wasn't their line of work, and they were often rather diffident men."[93] It is surprising to hear that men who developed a bomb to wipe out cities, that revolutionized not only their field but the world, that were at the top of their field with the admiring respect of legions of young scientists, would be shy. It is surprising that Alvarez, who witnessed the first three atomic bombs detonating, who lectured undergraduate physics for decades, who led large teams of brilliant physicists, who was on the board of directors of several large technology companies and CEO of his own optics company, could be shy. This impression does not come through in the written record or in communications with people under Alvarez who had witnessed Alvarez's wrath.[94] However, as Rhodes said of physicists, they are "in their own world."[95] Rhodes was not directly under Alvarez; his boss was Art Singer at the Sloan Foundation, and he referred to his time with Alvarez as "working with a Nobel laureate physicist named Luis W. Alvarez" and "When I helped

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92 Rhodes, *How to Write*, p. 51.
93 Rhodes interview by author, 20 May 2009, p. 25.
94 Philip Melese electronic correspondence with author, 12 Oct. to 14 Nov. 2009. Melese "was part of [Alvarez's] group 1980-1982." He once witnessed Alvarez's "merciless grilling of a graduate student."
95 Rhodes interview by author, 20 May 2009, p. 31.

Luis Alvarez organize his memoir."[96] Thus Rhodes had a chance to see Alvarez's personality without having to fear him. Rhodes said,

> Luis never would stand up in an audience and argue with the speaker. He didn't want to appear intelligent and I think that was some deep shyness on his part. At least some anxiety, some social anxiety, that his bluster and bravado as a tall, pilot, experimental physicist kind of works against—I think there was a lot of hesitation and shyness underneath in his character.
>
> At least I felt that when I worked with him, because I'd say, "Why don't you," and he'd say, "No, no, no, like don't even talk about that, I don't want to hear about that."[97]

Rhodes said that physicists tended to have that social anxiety. It surely fits some stereotypes about young people interested in the sciences. It matches many of the graduate students at the University of Texas's physics and astronomy programs, if we accept anecdotal evidence. Physicist Richard Feynman's popularity among physics graduate students derives at least in part from his outgoing nature and his advice on picking up women in his *Surely You're Joking, Mr. Feynman!* A chapter titled "You just <u>ask</u> them?" is self-explanatory.

But why Alvarez? Rhodes thought perhaps Alvarez's father helped inspire him to write an autobiography. Rhodes recalled "that Trower had been after him to do something like this for a long time,"[98] but he also thought that Dr. Walter Alvarez's career had a lot to do with it. Not to be confused with Luis's geologist son, also named Walter Alvarez, the elder Walter Clement Alvarez was a successful Mayo Clinic researcher who wrote a widely read weekly medical advice column. Rhodes remembered

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96 Rhodes, *How to Write*, pp. 16, 51.
97 Rhodes interview by author, 20 May 2009, p. 18.
98 *ibid*.

reading that column in his youth and pointed out that "His father was a writer. His father was a doctor. So many physicians are. A surprising number of physicians also write about their work."[99] Walter Alvarez also wrote two autobiographies, *Incurable Physician* in 1963 and *Alvarez on Alvarez* in 1977, as well as at least thirteen popular medical books.[100] Although no evidence has turned up, it is plausible that Walter's starting in on his second autobiography could have helped kick start Luis's effort at his own work. Luis Alvarez did say, "I apparently inherited a love for scientific history from my father."[101] Additionally, a sort of festschrift for his father went to press in 1976: *Walter C. Alvarez: American Man of Medicine*.[102] The timing does tempt one to wonder if Luis Alvarez saw his prolific father's success and was inspired to write for a public audience himself.

ALVAREZ'S LIFE OUTSIDE SCIENCE

We have seen that Alvarez developed a body of scientific work outside physics, but he had many interests outside science altogether. That should not be surprising, as everyone has hobbies, but Alvarez's ambition meant that his hobbies were of a grand scale. Some of his hobbies, such as playing piano, shooting three-cushion billiards, and mountain climbing did not occupy much of his time, but piloting, inventing, and involvement in the business world were among his more impressive achievements.

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99 *ibid*., p. 31.

100 Walter Clement Alvarez, *Incurable Physician* (Englewood Cliffs, N.J.: Prentice Hall, 1963), *Alvarez on Alvarez* (San Francisco: Strawberry Hill Press, 1977).

101 Alvarez autobiography draft dated 14 May 1972, p. 75. LWAP, box 46, folder "Autobiography p. 1-100."

102 David H. Scott, ed., *Walter C. Alvarez: American Man of Medicine* (New York: Pyramid Books, 1976).

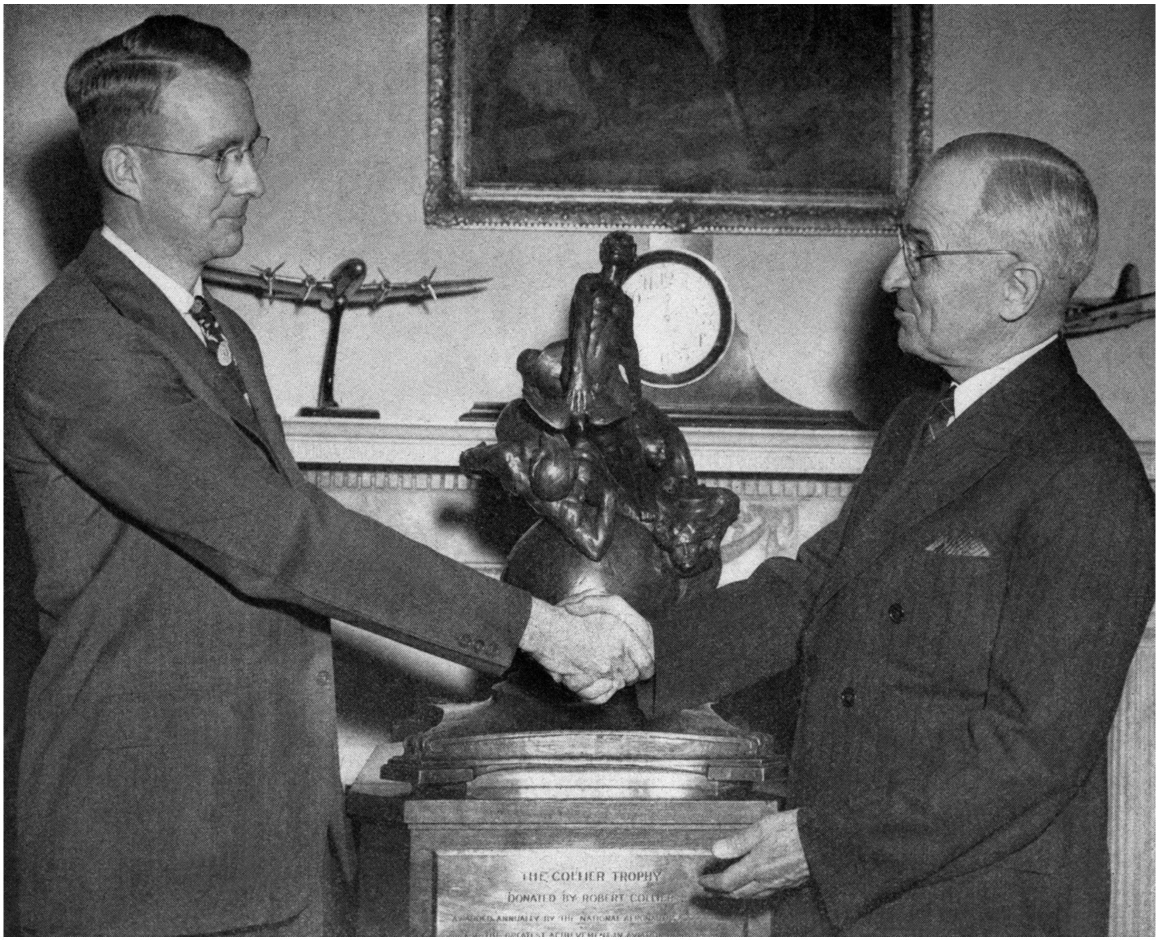

Figure 2.2: President Truman presenting Alvarez with the Collier Trophy, 1946.

Alvarez considered his piloting career to be as big a part of his life as physics. He flew for fifty years and owned his own Cessna 310. He won the Collier Trophy in 1945 for inventing the Ground Control Approach system letting flight controllers guide a pilot down in zero visibility conditions. Of course, that invention did not require Alvarez's piloting skills, but he logged over one thousand hours of flight and was the first civilian to land under hood using the GCA. He trained in the Navy instrument course, so he “could fly any plane, regardless of size.”[103] He had many exciting adventures in the cockpit, including this one in a plane of the same design as the *Enola Gay*:

> Once the pilot of a B-29 gave me the controls, and I put the plane into a well-coordinated 360-degree turn banked at 70 degrees, maintaining altitude to within forty feet. The pilot complimented me on my performance and asked casually if I knew the wings came off at 80 degrees.[104]

Alvarez met many famous people, but had only ever asked for an autograph from two: World War II ace and daredevil Jimmy Doolittle and Chuck Yeager, also a wartime ace and the first man to break the sound barrier. He was a very enthusiastic about flying: “I think of myself as having had two separate careers, one in science and one in aviation. I’ve found the two almost equally rewarding.”[105] Discussion of his flying appears on only five pages of his autobiography, but obviously, few people bought his book to read about his piloting experiences.

Alvarez was also an inventor with forty patents. He invented a detonator system at Los Alamos using an electrical pulse to explode a capacitor. They needed thirty-two detonators to go off as close to simultaneously as possible in order for the implosion design of the atomic bomb to work. Alfred Nobel’s electric detonators were accurate to roughly a millisecond; Alvarez’s detonators exploded to within a few billionths of a second of each other. His detonator is now the industry standard method. He did not profit from that invention, since it was invented under government contract with the standard one-dollar payment. Later, he unsuccessfully worked on color television

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[103] Alvarez, *Adventures*, p. 268.
[104] *ibid*.
[105] *ibid*., p. 31.

systems. He had a fascinating idea for a thin focusing lens made of two oddly shaped plastic elements that could change focal length by sliding the two past each other.[106] The Polaroid Company licensed the technology for a camera in 1986, but most companies waited for the patent to expire before using it. While on safari in Africa, Alvarez noticed that his movie camera was very difficult to stabilize at its maximum zoom. He devised a number of systems for stabilizing the image on the film without stabilizing the lens or camera body. Obviously, he also patented a number of particle accelerator and detector-related ideas, as well as a golf training stroboscopic device, an aircraft warning system, and various other inventions.

William Humphrey, Alvarez's graduate student and later president of Alvarez's ORDCO corporation, described the difference between academia and business: "Ivory towers are white and smokestacks are black." He thought the two were about as different as possible, but "Luis W. Alvarez is one of those rare people who can … successfully function in both environments."[107] Both his thin focusing lens and image stabilized camera systems led to Alvarez founding companies. Alvarez founded ORDCO, the Optical Research and Development Company, later named Humphrey Instruments. Another graduate student, Pete Schwemin, joined Alvarez to form another company, Schwem Technology with Alvarez as chairman of the board and chief inventor and his wife, Jan, as president. Besides the two companies he formed, Alvarez was a good friend

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[106] Alvarez, "Development of a variable-focus lenses and a new refractor," *Journal of the American Optometric Association* 49, no. 1 (Jan. 1978), p. 24. See also Alvarez patent: "Two-Element Variable Power Spherical Lens," No. 3,305,294; 21 Feb. 1970.

[107] William Humphrey, "Ivory Towers and Smokestacks," in W. Peter Trower, ed., *Discovering Alvarez: Selected Works of Luis W. Alvarez with Commentary by His Students and Colleagues* (Chicago: University of Chicago Press, 1987), p. 232.

of several influential business leaders that led to business relationships. Most inventors hope to get lucky enough to pitch an idea to a successful company; Alvarez just called his friend Chuck Percy, the president of camera makers Bell & Howell (Percy later became an Illinois senator). Alvarez was also a good friend of Edwin Land, the founder of the Polaroid Corporation, who he called Din: "I find it most interesting that two people who are so much alike in so many ways, started in physics at almost the same time, with nearly the same interests, and then spend most of our professional lives at opposite ends of the spectrum."[108] He listed a number of honors and memberships that they shared and "how nearly Din Land's career and mine had been mirror images, one of the other,"[109] then noted that Land was then worth half a billion dollars, while Alvarez, writing in 1972, had yet to make his first million. He wanted to be successful in business as well as in physics: "business is exciting and challenging, and as a hobby it certainly beats bridge."[110] Besides his own two companies, Alvarez was a consultant for IBM. He was friends with Dave Packard, who invested in Schwem, and Bill Hewlett of Hewlett-Packard. In 1957, "Bill and Dave invited me to sit on the small board of directors of the small Hewlett-Packard company,"[111] a post he kept for twenty-six years until he hit the mandatory retirement age. As Alvarez said, "Hewlett-Packard and its people deserve more mention here than they've had, which just shows that there are people in this world

[108] Alvarez autobiography draft dated 31 May 1972, LWAP, box 46, folder "Autobiography p. 1-100," p. 89.
[109] *ibid*., p. 92.
[110] Alvarez, *Adventures*, p. 274.
[111] *ibid*., p. 273.

called editors."[112] Alvarez's business adventures, like his piloting, are also relegated to a handful of pages in his autobiography because he is most well known as a scientist, but it is clear that they were a big part of his life. Alvarez cared about how he would be remembered, his personality included.

THE PERSONALITY OF LUIE ALVAREZ

Delving into Alvarez's long autobiography draft reveals much more than his career, but also his persona. Much of Alvarez's personality comes through when exploring his ethnicity in chapter six. His knowledge of German and French but not Spanish, his politics, and the fact that he was not disadvantaged all contribute to understanding his ethnicity and help us imagine his personality. Were he a Chicano, he might have been a very different person. Although his intense anticommunism was not necessarily part of his ethnicity, we saw that his decision to testify at Robert Oppenheimer's security hearings made him some enemies. Some of his personality comes through in chapter four on his scientific career outside of physics. He was very competitive, even with his son. We saw that his relationship with Walter could be difficult, but the sources do not reveal much about that. We might infer from his autobiography that he was very confident about his achievements; he certainly deserved to be proud. His autobiography mentions a few details in passing. For example, he was not religious.[113] What else could documents capture about his persona?

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[112] *ibid*.
[113] Alvarez, *Adventures*, p. 276, 279.

We know that many people did not like Alvarez. In an article in *American Scholar*, George Greenstein recalls someone calling Alvarez a "son of a bitch."[114] I have never heard a scientist called that in a journal. Did he deserve that type of insult? Surely everybody has rivalries and antagonists. Richard Rhodes suggested that his competitive nature could have fed into that image. Alvarez "was terribly competitive. People didn't like him at Los Alamos. I asked someone why, I don't remember who, and they said, 'well, if I was shooting pool by myself at the pool table, Luis would come in and say, "I can beat you." And then often he would.'"[115] At Los Alamos, Alvarez played a weekly poker game. He wrote that John von Neumann, "one of the world's great mathematicians … wrote the classic *Theory of Games and Economic Behavior* but usually left our poker games poorer than when he arrived."[116] Admittedly, that is something to be proud of. Berkeley physicist Emilio Segrè heard that Alvarez was fawning to his superiors but mean to his inferiors.[117] That type of aggressive competitiveness could surely rub some people the wrong way.

On the other hand, at least some of his graduate students adored him. Peter Trower pushed Alvarez to write his autobiography and organized and edited a festschrift in his honor. The rough manuscript of Alvarez's autobiography is covered with Trower's handwritten comments that are often fawning, sometimes embarrassingly so. Richard Muller, who contributed to the dinosaur extinction theory and developed his own

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114 George Greenstein, "Luie's Gadgets: A Profile of Luis Alvarez," *American Scholar* 61, no. 1 (1992), pp. 94- 95. Greenstein does not call him that, he only reports on hearing someone else calling him that.
115 Rhodes interview by author, 20 May 2009, p. 7.
116 Alvarez, *Adventures*, p. 130.
117 Emilio Segrè, *A Mind Always in Motion: the Autobiography of Emilio Segrè* (Berkeley: University of California Press, 1993), p. 135.

Nemesis theory, acknowledged Alvarez's "reputation among outsiders as a tough curmudgeon," but said that "Luie had obviously generated a great deal of loyalty and love among those who had worked with him."[118] He described his initial discomfort at calling Alvarez "Luie" as did everyone else at the Lawrence Radiation Laboratory. He recalled once breaking a large photomultiplier tube worth twice his annual salary as a research assistant, so he was sure he would be fired:

> A short time later I saw Alvarez, and confessed what had happened. "Grrrrr . . . reat!" he roared, and put out his hand as to congratulate me. He shook my hand vigorously, but I protested. "Welcome to the club," he continued. "Now I know you're becoming an experimental physicist." To become a real member of the Mafia you have to murder someone. To become an experimental physicist, Alvarez seemed to feel, you had to destroy some expensive equipment. It was a rite of passage. "Don't do anything differently," he advised. "Keep it up."[119]

Muller felt a sense of warmth and finally started calling him Luie. So the "tough curmudgeon" might only seem so to outsiders. Clearly Alvarez was not mean to all his inferiors, certainly not his own graduate students and post-docs. Alvarez summed it up: "I'm not universally loved. Some heartily dislike me. But all in all, I'm happy with my personal relationships."[120]

In his autobiography, Alvarez mentions his fraternity years as an undergraduate. He lived in the Phi Gamma Delta fraternity house at the University of Chicago, "a house somewhere in the middle of the social spectrum. … It was the center of my social life."[121] He mentioned his first love, Katherine Madison. In college, his summers were spent on

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118 Richard Muller, *Nemesis the Death Star: The Story of a Scientific Revolution* (New York: Weidenfeld & Nicolson, 1988), p. 29.
119 *ibid*., 24.
120 Alvarez, *Adventures*, p. 274.
121 *ibid*., p. 17.

his friend's "twenty-six-foot cabin cruiser, the *Lo-Ha-Lu*,"[122] and Alvarez was the navigator. Rhodes had the impression Alvarez was, "I think, a lady's man, from what I could tell. He loved his fraternity in college, [and] probably did some drinking in his time."[123] An image emerges of the young Alvarez as a whole person, not just a set of publications and a set of archival boxes. It also seems clear that Alvarez wanted his autobiography to be more than a scientific biography.

Alvarez described his first wife, Geraldine Smithwick, as "very pretty and very popular."[124] They married in 1936 and had two children, Walter and Jean, born four years apart to the day, as well as a stillborn child in between. Alvarez described the cold obstetrician who "offered no condolences" and showed Gerry "no interest in her as a person."[125] Nobody would fault Alvarez for leaving this out, but again, he wanted his autobiography to include his humanity. Walter became the respected geologist who collaborated with his father on the dinosaur extinction theory. Luis and Gerry realized their marriage was failing and wanted to stay together until the children were grown, but ended up divorcing in 1957 when Walt was a senior in high school. In 1958, Luis married Janet Landis, who had been a data analyst for Alvarez's hydrogen bubble chamber group and was twenty years his junior. She had worked in the Radiation Laboratory. He offered advice on staying together in his autobiography:

> The main difference between my first and my present marriage is in communication. During the war I worked under strict secrecy rules for five

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[122] *ibid.*
[123] Rhodes interview by author, 20 May 2009, p. 7.
[124] Alvarez, *Adventures*, p. 37.
[125] *ibid.*, p. 130.

> years; Gerry and I forgot how to share our lives. I was also too frequently away and couldn't afford long-distance calls, which were expensive then. Time has relieved that financial problem, and Jan and I have talked almost every night for three decades, transcontinentally and internationally. Neither of us has ever signed off because the bill was getting high. We talk for as long as we have something to say, and we consider the expense vital. Only from Antarctica, Vietnam, and Egypt did I not call Jan every night.[126]

Luis and Jan had two children, Donald and Helen, and remained married until his death in 1988. I cannot claim to have captured Alvarez's life in a few pages, but it does seem clear he wanted people to remember him beyond his work.

CONCLUSION

Whatever Alvarez's inspiration for establishing the historical record, he did it at the right time. Physics had risen to preeminence among the sciences, and Alvarez's interest in the history of science began before it became commonplace for physicists to care about their past as a social group. Rhodes described physicists' reluctance to write history: "Louie was very aware of that and amazed by it."[127] Rhodes added that before Alvarez, "There really was no tradition in science. There is a little bit more now." Alvarez had succeeded in convincing the world that the physicists' story was important.

Murray Gell-Mann eventually published *The Quark and the Jaguar* discussed above in 1994. Through all the suffering that the book seems to have caused him, one might wonder why he considered it so important to publish a popular work on a subject so esoteric as quarks. Rhodes had a suggestion. In 1977, another theoretical physicist, Steven Weinberg, published a best-selling popularization of his work on early cosmology

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[126] *ibid*., p. 281.
[127] Rhodes interview by author, 20 May 2009, p. 29.

titled *The First Three Minutes*.[128] In 1979, the Sloan Foundation published the first in its Science Book Series, the best-selling *Disturbing the Universe* by British-American physicist Freeman Dyson.[129] Such popularizations were certainly not new to physics. As early as 1686, Bernard le Bovier de Fontenelle wrote a vernacular popularization of heliocentrism, *Conversations on the Plurality of Worlds*. Einstein, although he did not write an autobiography, did write a popularization of his special theory of relativity in 1916. However, something was in the air in the American physics community in the 1970s. Weinberg was on the committee that picked the Sloan "Science Book Series" of scientist autobiographies that included Alvarez's. His popularization started a trend. Rhodes argued, "That's when Murray Gell-Mann decided he needed to do the same thing … since he knew he was smarter than Steven Weinberg. Of course he didn't make it happen and it really bothered him a lot. His book was not the successful book" that Weinberg's was.[130] Weinberg went on to write several popular books, although none sold as well as *The First Three Minutes*. Rhodes argued that the even more successful *A Brief History of Time* by Stephen Hawking was part of that tradition, but "Weinberg was the one who really broke the new ground on that."[131] Hawking is British, Weinberg is American; American physics was exerting its reach.

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128 Stephen Weinberg, *The First Three Minutes* (New York: Basic Books, 1977). A physics graduate student at the University of Texas told this author that a visiting lecturer giving a talk said that when he first heard of this book, he thought it was about Weinberg's sex life. Weinberg, sitting in the first row, was not amused.
129 Freeman Dyson, *Disturbing the Universe* (New York: Harper & Row, 1979).
130 Rhodes interview by author, 20 May 2009, p. 29.
131 *ibid*., p. 30.

"Almost everyone who writes an autobiography feels a need to explain why he indulged in such egocentric behavior."[132] So started the introduction to Alvarez's autobiography, *Alvarez: Adventures of a Physicist*. The shy physicist who wanted to stay in the background was also an early advocate for placing physicists in the history books. His desire to make a name for American physics was preceded by Robert Millikan by some decades, but it seems that in Millikan's time, physicists were satisfied to take their place among other sciences. In the 1970s, physics was at its peak of influence. It was also on the verge of beginning its relative decline.

We focus on autobiographies because of what they say about physicists' perception of the public perception of physics. Today's American physicists care what the public thinks of them as a group. Freeman Dyson's contribution to the Sloan Foundation Series is partly autobiography, partly popularization. He explains,

> A substantial part of this book is autobiographical. I make no apology for that. It is not that I consider my own life particularly significant or interesting to anybody besides myself. I write about my own experiences because I do not know so much about anyone else's. Almost any scientists of my generation could tell a similar story.[133]

Despite making no apology, Dyson sounds apologetic. He hopes that he will remove persona from the story of physics, which is the story of the universe. He believes that he is exposing capital-T Truth, not simply some individual's take on reality. We focus on Alvarez largely because he was telling his personal story. He had become comfortable "engaging in such egocentric behavior."

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[132] Alvarez, *Adventures*, *xi*.
[133] Dyson, *Disturbing the Universe*, p. 6.

Chapter 3: ***Alvarez's Sense of History: The Textbook***

> I am often surprised to find how little some of my colleagues know of the men whose theories and experiments they teach in their courses year after year.[1]
>
> - Luis Alvarez on physicists and history

The world's first two cyclotrons—the ones that revolutionized physics—languished "in the back of a drawer"[2] at Berkeley when Luis Alvarez arrived in 1936. Ernest O. Lawrence invented these four inch[3] cyclotrons, the particle accelerators that earned him the Nobel Prize; that led to an explosion in particle physics discoveries; that launched the careers of Nobel Laureates Edwin McMillan, Melvin Calvin, Glenn Seaborg, and Alvarez; that established the University of California and its Radiation Laboratory as premier physics centers; and that started a new way of doing physics later described as "big science."[4] Lawrence was Alvarez's mentor and hero, so it seems natural that when he found Lawrence's first, hand-made cyclotrons, along with some

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[1] Luis Alvarez autobiography draft dated 14 May 1972, The Luis W. Alvarez Papers, BANC MSS 84/82 cz, The Bancroft Library, University of California, Berkeley (henceforth LWAP), box 46, folder "Autobiography p. 1-100," p. 75.

[2] Luis Alvarez, *Adventures of a Physicist* (New York: Basic Books, 1987), p. 46.

[3] These cyclotrons—two were made of this size—are variously described as being four or six inches, which may be alternate measurements of the six inch magnets and of the four inch vacuum chambers they contain.

[4] For more on the significance of "big science," see Peter Galison and Bruce Hevly, *Big Science: the Growth of Large-scale Research* (Stanford: Stanford University Press, 1992). For more on the history of this laboratory, see John L. Heilbron and Robert W. Seidel, *Lawrence and His Laboratory: A History of the Lawrence Berkeley Laboratory, vol. 1* (Berkeley: University of California Press, 1989). For more on the success of U.C. Berkeley's physics department's approach to research, see Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: Chicago University Press, 1997).

research notebooks by Lawrence and co-inventor Stanley Livingston, he arranged to put them on public display.

What is striking is that they should have been forgotten in the first place. Alvarez "naturally recognized them for their importance,"[5] but "if I hadn't found them … they would certainly have been thrown away."[6] In 1934, Alvarez took time off from graduate school at the University of Chicago to visit Berkeley and learned that the little cyclotrons "were quickly discarded as Ernest and Stan Livingston pushed on to their goal of one million electron volt protons, which was achieved with the eleven-inch cyclotron."[7] He continued:

> It is probably fortunate for the history of science that these treasures were found by someone with archeological instincts, rather than by someone who could easily have been given the job of "cleaning the junk out of Room 229."[8]

In the 1930s, most American physicists were only interested in the new, the cutting edge and not in the outmoded. When a physicist from Whittier College in Los Angeles mailed physics department chair Raymond Birge for information on the Berkeley physics department, Birge replied that Berkeley physicists Ernest Lawrence, Leonard Benedict Loeb, and Robert Bigham Brode "are none of them interested in the history of physics, and so I doubt if you could get much help from them."[9] They had no sense of history. Alvarez was one of the first physicists with these "archeological instincts." The

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[5] Alvarez interview by Charles Weiner and Barry Richman on 15 Feb. 1967, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, session II, p. 1.
[6] Alvarez, *Adventures*, p. 46.
[7] Alvarez, "Berkeley: A Lab Like No Other," *Bulletin of the Atomic Scientists* 30, no. 4 (Apr. 1974), p. 22.
[8] *ibid*.
[9] Raymond Thayer Birge to Alfred Romer, Whittier College, 15 Jan. 1940, Raymond Thayer Birge Papers, BANC MSS 73/79 c, The Bancroft Library, University of California, Berkeley, box 40, folder 3.

discovery of the cyclotron and notebooks excited Alvarez: "I was able to relive, vicariously, the excitement that Ernest and Stan experienced in those days, by studying the notebooks carefully."[10] Surely even today there are forgetful scientists, ones so focused on their work that they forget to preserve the artifacts and notes of their discoveries, but throwing away history like that is unlikely. Physicists today are intensely mindful of their place in the pecking order and in history. Physics in 1936 was a different beast, but they should have been a bit curious about where their work should have led. Alvarez told Richard Rhodes that physicists thought of the first cyclotron, "we don't need this anymore," but after Alvarez rescued it, "It's in the Smithsonian, of course."[11] Alvarez explained that "The circular magnetic machines at NAL [the National Accelerator Laboratory at Fermilab] and CERN (the European Center for Nuclear Research) are direct descendants of the four-inch cyclotrons, just as the eleven-inch cyclotron was."[12] The CERN laboratory near Geneva that Alvarez referred to is now the home of the Large Hadron Collider that has grabbed the attention of the popular press in recent months for being the world's largest atom smasher at seventeen miles in circumference. Luis Alvarez was one of the leaders in making a place for physicists in history.

Before other physicists had fully developed an identity as physicists, Alvarez was already interested in identifying the achievements and heroes of modern American physics, himself included. English physicists had a long tradition. The Lucasian Chair of

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[10] Alvarez, "Berkeley: A Lab Like No Other," p. 22.
[11] Richard Rhodes interview by author, 20 May 2009, p. 29.
[12] Alvarez, "Berkeley: A Lab Like No Other," p. 23.

Mathematics, which Stephen Hawking held until his recent retirement, derives its prestige not from the work of Cambridge physicists of a century ago; is the post that was held by Sir Isaac Newton in the seventeenth century. It is easy to forget that American physics, although it derives from the European tradition, had to establish itself first as a viable field in academia, then as a preeminent field in the public's eye. The first job, to prove that American physicists could do world-class work, was done by physicists like Josiah Willard Gibbs, Henry A. Rowland, Albert Michelson, and Robert A. Millikan in the late nineteenth and early twentieth centuries.[13] The second job was twofold: to show the public, government, and industry that physicists are valuable and to convince American physicists of their own self-worth, their own identity.

Alvarez's interest in the history of physics is most innovative in an unfinished draft of a physics textbook. Around 1952, Luis Alvarez began work on an atomic physics textbook that he left a little more than half-done. This textbook draft took a historical approach that is now common, but was then quite new. Physics textbooks in the 1940s had begun to transform from dry descriptions of physical apparatus and phenomena to colorful, illustrated texts full of humanizing descriptions of the men and women behind the formulas. Before the 1940s, textbooks only included current physics theories; disproven theories were discarded like some old cyclotron that could not produce the desired energy levels. While Alvarez did not invent this move towards including some history of physics, his text would have been an early example of this

[13] This story is told authoritatively in Daniel Kevles's *The Physicists: The History of a Scientific Community in Modern America* (Cambridge, Mass.: Harvard University Press, 1995). Originally published in 1977, the new edition includes an excellent discussion of the decline of physics since the book's original publication.

approach. Alvarez's third and sixth chapters are most striking in the way they interweave physics with its history. Chapter one, on relativity, is odd in that its presentation is not as historical as those of the other chapters, but comparing it to other textbooks of that time points out the dangers of lay writers of physics textbooks. Alvarez provides a simple way to relate to Einstein's theory. Chapter four of his text is interesting in its description of a misunderstood episode in the history of science—the development by J. J. Thomson of what is now known as the "plum pudding" model of the atom. This chapter will focus largely on that textbook draft contained in the Luis W. Alvarez papers at the Bancroft library of the University of California, Berkeley.

Like the rest of this dissertation, this chapter requires a reminder to the reader that physics was not a prestige science early in Alvarez's career. Physics dominated the sciences from the end of the Second World War to the late nineteen seventies, but early in Alvarez's career, in the years before Hiroshima, physics was a small, specialty field. That can be measured in many ways, and many historians have noted this with information about publication rates, funding, and the like.[14] This approach will note another measure of prestige: the development of an American physics identity. As this chapter opens, physicists did not see themselves as important players in the science community, playing second fiddle to the chemists who had revolutionized warfare in the First World War. By the close of this chapter, physics was the premier science and the members of that community had an identity, a history, and heroes. That is the thrust of this chapter and the thesis of this dissertation.

[14] Perhaps the best example is Kevles, *The Physicists*.

The History of Physics Textbooks

As Alvarez recalled, "I went to the University of Chicago in the belief that I would probably become a chemist. All the popular-science books I had read in high school praised the lives and great deeds of chemists."[15] Physics textbooks in 1928 did not do the same for physicists. Alvarez tried to remedy that in a draft of his unfinished physics textbook that exposes a subtle transition in physics teaching. At first glance, the half-finished textbook draft started December 1952 looks like any other physics textbook once one sees past the façade of typewritten text and hand-drawn illustrations. It does have a few interesting alternatives to the usual formulation, for example, it uses a different approach to deriving Einstein's $E=mc^2$. However, the textbook's place in history comes from a comparison to the physics textbooks of its day. The untitled Alvarez textbook was one of the early physics texts to contextualize the ideas in history and tell the stories behind the equations. Of course, Alvarez did not do this single-handedly. His textbook was part of a trend that has roots as far back as 1906, but it is notable for two reasons. Alvarez's textbook, had it been published, would have pushed the trend toward historicism further and second, its rough form and hand-written comments reveal the intent to historicize physics. The Alvarez textbook told the story of the men behind the physics.

The chemistry book Alvarez cited as inspiration was Edwin Slosson's 1919 *Creative Chemistry*.[16] This popularization was the type of science boosterism that

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[15] Alvarez, *Adventures*, p. 16.

[16] Edwin Slosson, *Creative Chemistry* (New York: The Century Co., 1919).

physics would not see until after the Second World War. Chemists had a different relationship with the everyday world, perhaps making popularization easier. Slosson enthusiastically argued that chemistry produced tangible results in American lives as far back as Columbus, even further. Slosson described the history of metallurgy, the efforts to extract useful chemicals from corn, and the science of feeding the soil that also led to the development of explosives. The First World War is often called "The Chemists' War" and Slosson did not hesitate to discuss the role of his science in that war. He was much more ambitious than that, describing the history of man as having three stages: finding useful materials, adapting materials to our use, and creating artificial materials. This characterization places chemistry at the center of human endeavors, making chemistry the epitome of civilization. He pronounced these truths with a mellifluous, literary style worthy of a Yeats or Byron that may have found fertile soil in the florid scribblings of that time, but sounds pretentious today, does it not? He closed the work with a description of the energy that comes from radioactive radium, noting that extracting it would mean "all our troubles would be over," but concluding that "The atom is as much beyond our reach as the moon. We cannot rob its vault of the treasure."[17] By the time we did exactly that—extract power from the atom and walk on the moon—physics would be at its peak, but in 1919, chemistry was king and its practitioners told stories of heroes and legends well in advance of the same developments in physics textbooks.

[17] *ibid.*, p. 296.

Through the first half of the twentieth century, physics textbooks slowly introduced histories of physics. Before then, physics textbooks were drab lists of theories and formulas with little discussion of the men who created the ideas. Only those with the stature of a Galileo or Newton warranted any discussion, and one suspects that Galileo's battles with the Church made him an irresistible character. These textbooks resembled monographs in formatting, with minimal design but a lot of equations and a few line drawings of physics instruments. Some of this visual change seems to have affected the entire textbook industry. Frances Fitzgerald's *America Revised: History Schoolbooks in the Twentieth Century* notes the design evolution in history textbooks: when compared to the then-modern texts of the late nineteen seventies, early twentieth century texts

> look as naïve as Soviet fashion magazines … illustrations tend to be Socialist-realist-style drawings (there are a lot of hefty farmers with hoes in the Colonial-period chapters) or incredibly vulgar made-for-children paintings of patriotic events … by contrast, the current texts are paragons of sophisticated modern design. They look not like *People* or *Family Circle* but, rather, like *Architectural Digest* or *Vogue*."[18]

Certainly, the textbook industry progresses like any other industry, and there are trends that span across disciplines. However, physics texts add another twist. Physics textbooks from the nineteen forties to the sixties add a physics lore, that is, a mythology based on history but distinct from it in that it does not always come from a careful study of the history of science; it comes largely from an oral tradition that physicists develop both by word-of-mouth and from earlier textbooks. Part of the process of becoming a physicist is the physics training to be sure, but part of that enculturation is learning the oral tradition.

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[18] Frances Fitzgerald, *America Revised: History Schoolbooks in the Twentieth Century* (Boston: Atlantic-Little, 1979), p. 14.

How can one know that the tradition passed on orally is distinct from a historian's version? The two stories do not always agree. The physics lore has myths like the plum pudding story told later in this chapter.

Despite some efforts to understand textbooks in the humanities, they are often overlooked as a source in the history of science. Perhaps the best effort in this field so far is *Pedagogy and the Practice of Science*, edited by David Kaiser, but even he notes that "scholars in science studies have not dedicated much systematic attention to the topic."[19] That collection of essays is an excellent start, but it focuses more on French chemistry textbooks in the nineteenth century than physics textbooks in America. Two chapters on German physics textbooks offer tantalizing ideas, but our present topic requires more research. A dissertation by Rachel Hinckley is useful in that it shows how cultural meaning can be communicated even in mathematical textbooks, where she finds morals about hard work and capitalism.[20] The closest to a systematic look at American physics textbooks in the period here is a dissertation by science educator William Schick on the history of electricity and magnetism textbooks.[21] Schick derives his conclusions from the study of a sample of fifty physics textbooks and a closer investigation of a small subset of four textbooks in each of several periods. However, Schick's sample size is not exhaustive enough. He says that

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[19] David Kaiser, ed., *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives* (Cambridge, Mass.: The MIT Press, 2005).
[20] Rachel Fancelia Hinckley, "American Culture as Reflected in Mathematical Schoolbooks" (EdD diss., Columbia University, 1949).
[21] William Schick, "Electricity and Magnetism as Presented in American Secondary School Textbooks From 1800 to 1950" (Ed.D. diss., Columbia University, 1965).

> the style of the physics textbook became less formal around 1920. This trend is exemplified in the textbook by Black and Davis, *Practical Physics*. The prose is conversational, and there are numerous illustrations. Several of these are of famous scientists.[22]

In a footnote, Schick says, "This change may, in part, be due to the report *Reorganization of Science in Secondary Schools*"[23] without giving details. That 62-page government report, published in 1920, gives very general suggestions for science teachers.[24] In looking for the origins of history in American physics texts, it was not difficult to find numerous textbooks before 1920 with illustrations of famous physicists going back to Robert Millikan and Henry Gordon Gale's *A First Course in Physics* of 1906. That textbook has the same type of portraits and short biographies of prominent physicists, and also fails to connect them to the narrative of the surrounding text. It is impossible to prove a negative, so how can we assert that a given text was the first to do anything and ignore the possibility that we simply missed an earlier text?

Here we will build on the work of Kaiser, Schick, Fitzgerald, and Hinckley, but also on a collection of textbooks held at the University of Texas Pickle Research Center. The state of Texas keeps an archive of every textbook used in the state, including a collection of 436 physics textbooks dating back to 1808. Removing duplicate copies and teacher's editions, the collection has 291 physics textbooks. This collection, held in cold storage, is much larger than the sample Schick used, which is helpful, but the primary value of these books is that the Texas State Board of Education and its Texas Essential

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[22] *ibid*., p. 116.
[23] *ibid*.
[24] Department of Education, *Reorganization of Science in Secondary Schools* (Washington: Government Printing Office, 1920).

Knowledge and Skills guideline, or TEKS, largely determined national textbooks standards.[25] This peculiar circumstance has roots in the rise of the religious right and their decision to infiltrate grass-roots state education: Reverend Pat "Robertson's protégé, Ralph Reed, once said, 'I would rather have a thousand school-board members than one president and no school-board members.'"[26] The TEKS standard was developed in 1998, so the Pickle collection is an excellently representative sample of American physics textbooks since then, a good sample in the decades before then due to the size of the Texas textbook market, and gradually a less representative sample the further one delves into the older end of the collection. One might not rely on it for textbooks of the early nineteenth century, but it should suffice for textbooks of the middle of the twentieth, if only for its depth.

Even before TEKS, the Texas State Textbook Committee was very influential on the national scale due to the size of the Texas textbook market and sheer political will of Texans. Fitzgerald illustrated its influence decades before TEKS:

> For example, in 1961, a right wing fringe group called Texans for America intimidated the committee, and it pressured several publishers to make substantial changes in their American-history and geography texts.[27]

The effect was striking:

> Because of the Texas State Textbook committee, New England children, whose ancestors heartily disapproved of the Mexican War, have grown up with heroic tales of Davy Crockett and Sam Houston.[28]

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[25] Russell Shorto, "How Christian Were the Founders?" *New York Times*, 11 February 2010.
[26] *ibid*.
[27] Fitzgerald, *America Revised*, p. 34.
[28] *ibid*.

Textbook editor James Reid wrote a memoir of his years working at textbook publisher Harcourt Brace from 1924 to 1960 and corroborates the centrality of the Texas book market:

> Texas was the largest of the state adoptions. … It was in the thirties even more desirable than California, which for many years printed its own state-adopted elementary textbooks by renting the plates from the publishers. … Texas was the big plum, the most wanted adoption of all.[29]

Harcourt Brace stayed out of the California market but did what it could to sell books in Texas. This is still no guarantee that we will not miss any seminal text, but the Pickle collection provides a solid and representative sample. Of course, any extra textbooks discovered that are not in the Pickle collection are a welcome addition.

A look at the history of textbooks reveals a gradual transition from a just-the-facts approach—a list of theories and their equations—to colorful stories about physicists, literally and figuratively. The design changes noted above add color to illustrations that were simple and sparse in earlier texts as well as photographs nowhere seen in those earlier books, but as Schick points out, textbooks were faster to incorporate new technology than new physics.[30] Some textbooks of the nineteen twenties add short biographical sketches of major physicists. It seems surprising that the earlier textbooks did not include such information since the theories and formulas that comprise them are named after people. Physics concepts, equipment, and equations have names like Newton's laws, Maxwell's equations, and Planck's constant, implying authors whose

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[29] James M. Reid, *An Adventure in Textbooks* (New York: R. R. Bowker Company, 1969), p. 64. His story about trying to sell textbooks at a meeting in Austin's Driskill hotel is very entertaining. Jovanich was added to the company's title after Reid left.
[30] Schick, "Electricity," p. 132.

efforts presumably give insight into their achievements. It is slightly disjointed to discuss a named theory and not mention its namesake, yet that was standard practice. Sometimes, the naming conventions in physics strained that distant style. For example, when discussing Roemer's method for determining the speed of light, many textbooks would acknowledge that there was a man named Roemer and that he developed an eponymous method in 1676. Even then, the textbooks often left out Ole Roemer's first name, much less any biographical information. Historian Kathryn Olesko noted that in the nineteenth century at the University of Göttingen, even the equipment was named after the instrument maker: "Proper names attached to instruments reminded the students that the instrument was linked to a specific protocol that was reproduced, criticized, and improved upon in practical exercises."[31] The early adopters of the new style in textbooks give short, obituary-style biographies set apart from the text, often in a footnote, often on a sidebar, as if to clarify that it is optional reading. Both the biographical textbooks and their immediate predecessors organize their theories as if they were a summarized compilation of important physics journals simplified to the needed grade level. Like a physics journal, they are impersonal with little data about the author of a given theorem.

In 1906, Robert Millikan and Henry Gordon Gale, both at the University of Chicago, published *A First Course in Physics* with illustrated biographies of prominent physicists.[32] Their preface emphasized their new approach to pedagogy and did not

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[31] Kathryn M. Olesko, "The Foundations of a Canon: Kohlrausch's *Practical Physics*" in Kaiser, ed., *Pedagogy and the Practice of Science*, p. 342.

[32] Robert Andrews Millikan and Henry Gordon Gale, *A First Course in Physics* (Boston: Ginn & Co., 1906).

mention history, but Galileo's image and one-paragraph biography prominently faced the title page. These biographies were not integrated into the text and were set apart from the text on their own pages. Alvarez was a student at Chicago from 1928 to 1936, earning his bachelor's, master's, and doctoral degrees there. Millikan had long since moved to the California Institute of Technology, but Alvarez did meet Gale, who was dean. Gale offered to let Alvarez use optical grinding equipment usually reserved for technicians. In an interview, Alvarez said, "Dean Gale and I got to be very good friends. He was sort of my sponsor and patron, and I was his protégé, and he got me scholarships for the next two years and then four years of fellowships."[33] Alvarez was often close to the center of modern American physics.

In January 1940, Harvey E. White's *Classical and Modern Physics* introduced a new approach. White's preface softens the transition, giving the impression that the historical tone is in line with the practice of the time:

> One of the features of the book, and one that tends to make physics a more interesting subject, is the frequent insertion, as footnotes of short biographies of famous men of science. In addition to this a complete list of Nobel Prize Winners in physics is given in the Appendix. Each name is followed by a brief statement of the particular discoveries for which that person is noted and for which he or she was granted this most honored award.[34]

As noted above, physics textbooks as early as 1906 had been inserting short biographical notes. However, White's approach goes beyond this. He hints at this almost in passing: "The treatment of modern physics as it is presented here is given more or less in the order

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[33] Charles Weiner and Barry Richman interview with Alvarez, 14 February 1967 (Session I), p. 3.

[34] Harvey E. White, *Classical and Modern Physics* (New York: D. Van Nostrand, 1940.), p. *vi*.

of its historical development."[35] *Classical and Modern Physics* is a departure in tone, making physics a story with characters: physicists developing ideas over time. This change is most notable in the sections on modern science, partly because the sections on radiation and the atom were being rewritten almost constantly since the *fin de siècle*, so White had fewer examples of how to write them, but also because White was a professor of physics at the University of California at Berkeley. He was surrounded by many of the characters described in his textbook.

White wrote that his textbook covered the new physics in a historical manner: "first, a brief historical account of the discovery," followed by an explanation and applications.[36] This is a rational approach, given that the field was only forty-five years old and perhaps not yet organized by what Auguste Comte called "'the dogmatic order' in contrast to the 'historical order.'"[37] Comte argued the historical order was impossible in any well-developed field of science, but it seems like a natural way to recount very recent developments in physics. The classical physics sections in White's textbook proceeded much like earlier textbooks, giving the impression that physicists simply did not yet have another way of organizing the discoveries since 1895.

White dedicated *Classical and Modern Physics* to Ernest O. Lawrence, Luis Alvarez's mentor and head of the physics department at Berkeley. One sign of White's allegiance to science education and to Lawrence, whom he called "a close personal

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[35] *ibid.*, p. *v*.
[36] *ibid*, p. 5.
[37] Antonio García-Belmar, José Ramón Bertomeu-Sánchez, and Bernadette Bensaude-Vincent, "The Power of Didactic Writings: French Chemistry Textbooks of the Nineteenth Century," in David Kaiser, ed., *Pedagogy and the Practice of Science* (Cambridge, Mass.: The MIT Press, 2005), p. 220.

friend," is that he was the first director of the Lawrence Hall of Science, a museum focused on science education dedicated to Lawrence's memory.[38] White thanked Alvarez in the preface, along with Robert Oppenheimer, Lawrence, and six other Berkeley faculty members. While the intent here is not to imply that Alvarez was responsible for the new style of textbook, he seems to have been in a department very aware of the public image of physics. Alvarez noted that Lawrence was a great leader but often made high-energy physics work difficult because he insisted the physics department share time at their particle accelerators, or "beam time," with other departments, especially medical researchers who helped pay the bills. "We recognized the importance of these activities, but after spending days in cyclotron repairs we grumbled when a physiologist or a biologist turned up to claim the fruits of the first bombardment."[39] So strong was Lawrence's dedication to public relations that he once turned off a cooling system so that a visiting scholar could see how Alvarez and his crew fixed a shattered glass support.[40] That visiting scholar was Sir John Cockcroft, who was sent by Lord Rutherford of Cambridge University's famed Cavendish Laboratory, but Alvarez saw it as close to "cruel and unusual punishment."[41]

Another influence may be White's publisher, D. Van Nostrand. This New York based publishing house seems to come up again and again in the search for historicized physics textbooks. Besides White's 1940 textbook, D. Van Nostrand published several

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[38] Rolland B. Bartholomew, "Talks With Great Teachers: Harvey White." *The Physics Teacher* 21, no. 1 (Jan. 1983), p. 94.
[39] Alvarez, *Adventures*, p. 56.
[40] *ibid.*, 57.
[41] *ibid.*

other textbooks at the vanguard of humanizing physicists. White's textbook of 1959 is a key example.[42] Another textbook by Elmer E. Burns with Frank L. Verwiebe and Herbert C. Hazel, *Physics, A Basic Science*, published by D. Van Nostrand in 1943, significantly added to the momentum of White's 1940 textbook. Efforts to communicate with the company have been unsuccessful, but Nostrand president Edward M. Crane did publish a short book on the hundredth anniversary of their founding, *A Century of Book Publishing 1848-1948*.[43] This volume has precious little information about any intentional shift toward historical physics textbook, but does describe the company's early efforts in physics such as a primer on Einstein's theory of relativity, *From Newton to Einstein* by Benjamin Harrow, published in 1920.[44] Crane says this general audience book "gained a spectacular circulation"[45] and seems to have encouraged D. Van Nostrand to pursue physics textbooks. The company's early efforts in physics textbooks were greatly boosted with their publication of *X-Rays and Electrons* by Arthur Holly Compton in 1926.[46] Compton was Alvarez's graduate advisor at the University of Chicago, where Alvarez finished his Ph.D. in 1936. Once again, Alvarez seems to have had quite a vantage point to observe history.

In 1943, Elmer E. Burns justified his historical approach in *Physics: A Basic Science*, complaining that in other textbooks, "there is scarcely a word about the

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[42] Harvey Elliott White, *Physics: An Exact Science* (Princeton: D. Van Nostrand, 1959).
[43] Edward M. Crane, *A Century of Book Publishing 1848-1948* (New York: D. Van Nostrand, 1948).
[44] Benjamin Harrow, *From Newton to Einstein: Changing Conceptions of the Universe* (New York: D. Van Nostrand Company, 1920).
[45] Crane, *A Century of Book Publishing*, p. 53.
[46] Arthur Holly Compton, *X-Rays and Electrons* (New York: D. Van Nostrand, 1926). While the publication data in Compton's book says 1920, every other reference says 1926, including Crane, *A Century of Book Publishing*, p. 53.

achievements of Faraday whose discoveries have changed our way of living."[47] His approach largely followed White's, introducing history when possible, but also spending more time on history in the sections on modern physics. Again, this period in the history of physics was most amenable to the historical approach, and the fact that it was new to textbooks allowed writers more leeway in that they did not have to follow in the footsteps of centuries of textbook descriptions of Newton's physics. Writers in the mid-twentieth century could cover modern physics as they saw fit. What makes Burns' textbook interesting is that it is explicitly, self-consciously historical. Alvarez would one-up him.

Both Elmer Burns and Harvey White published their first historically minded physics textbooks while the atomic bomb was being developed. It may be tempting to argue that they were influenced by Leo Szilard's famous 1939 letter to Franklin Roosevelt asking the president to begin work on an atomic bomb, but that letter was quite secret and White would have been nearly done writing by then.[48] Additionally, textbooks generally take three to four years or more to write, so White and Burns could not have been reacting to something so recent. White was in the Berkeley physics department, along with Alvarez, Lawrence, and Oppenheimer, but it seems unlikely that Burns was aware of these developments. According to the title page, Burns was an emeritus teacher of physics at Austin High School in Chicago, not exactly at the center of the cutting-edge physics research community. His first coauthor as Frank L. Verwiebe, Associate professor of physics at Hamilton College in upstate New York, formerly associate

[47] Elmer E. Burns, *Physics: A Basic Science* (New York: D. Van Nostrand, 1943), p. *v*.

[48] For more on the Szilard letter and the Manhattan Project, see Richard Rhodes, *The Making of the Atomic Bomb* (New York: Simon and Schuster, 1986).

professor of physics at Eastern Illinois State Teachers College, again not at the cutting edge of physics. Finally, they were joined by Herbert C. Hazel, a Major in the U.S. Marine Corps, formerly head of the science department at Bloomington High School and assistant professor of physics at Indiana University. Attempts to find out whether Alvarez read Burns have been unsuccessful, but he was clearly in contact with White, since White thanks him for giving suggestions on his manuscript.

Instead, it was the launch of Sputnik in 1957 that helped standardize this historical approach to physics textbooks. This story is widely discussed in the literature; suffice it to say that science education became a national priority after Americans became convinced that science and engineering education were the solution to the perceived "missile gap."[49] After having helped introduce the style, in 1959 Harvey White returned with a more thorough approach with his *Physics: An Exact Science*, published by the same company, D. Van Nostrand. Perhaps the greater attention paid to basic physics after Sputnik had an effect on textbook funding; it shows in *Physics: An Exact Science*. White's new textbook elaborated on the types of history that Burns explored, and added a lot of illustrations and elaborate formatting missing from the earlier just-the-facts textbooks. Alvarez's textbook would have been published before this, so it is fair to argue that Alvarez was ahead of the curve on the approach, but it is not clear what type of typographical flair his typewritten manuscript and hand-drawn illustrations would have received.

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[49] Kevles discusses it in *The Physicists*, pp. 384-6, Fitzgerald discusses it in *America Revised*, pp. 179-89, but there are countless sources on Sputnik and American education.

In 1960, Alvarez was involved in another textbook project that helped transition to modern storytelling in physics. Alvarez was a member of the Physical Science Study Committee (PSSC) that produced *Physics*, although he does not appear to have been directly involved in that text. In 1957, Harvard physicist Jerrold Zacharias created the PSSC to develop high school physics films and soon branched out to textbooks with funding from the National Science Foundation (NSF).[50] By 1975, the PSSC had received $101,207,000 from the NSF for fifty-three projects, including an influential textbook, *Physics*.[51] White described the change represented by the PSSC textbook as a transition from experimental verification of established laws and principles to discovery of those laws and principles.[52] I have elsewhere noted the transition in physics textbooks around this time from depicting experiments to depicting nature.[53] Alvarez narrated an educational film for the PSSC, "What Is Physics"[54] and may have been worked on their textbook, but it has been difficult to determine his level of involvement.

This uncertainty should not be completely discouraging. Alvarez clearly worked a great deal on his 1952 textbook, and there is little evidence of that in his papers, besides the text itself. One would expect a fair amount of correspondence with publishers and possibly other physicists. One problem is the volume of correspondence; another is the

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[50] Dorothy Nelkin, *Science Textbook Controversies and the Politics of Equal Time* (Cambridge: Cambridge University Press, 1977), p. 22.
[51] *ibid*., p. 22 and appendix 1, "NSF Precollege Science Curriculum Project Grants in Thousands of Dollars (1957-1975)."
[52] White, Autobiography manuscript, chapter K "Special Lectures and Trips," p. 2. Harvey Elliott White Autobiography Manuscript, BANC MSS 81/169, The Bancroft Library, University of California, Berkeley.
[53] Rubén Martínez, "Plum Pudding and the Folklore of Physics," unpublished manuscript.
[54] Dan Wilkes to Alvarez, 3 June 1958 letter and manuscript for film, LWAP box 65, folder 11, "Physical Science Study Committee."

nature of the textbook publishing industry. As Fitzgerald says, "there is a great deal of secrecy in the textbook business. Not just the publishers and editors but the authors as well do not care to explain exactly how texts come into being."[55] She explains the complexity:

> The making of a text is a complicated business—much more complicated than the making of a trade book. Texts are not "written" anymore; they are, as the people in the industry say, "developed," and this process involves large numbers of people and many compromises. Not since the twenties have textbook publishers commissioned a basic history text from a singe author and simply printed it, as they would a trade book. The costs are too great, the risks too high, and the demands from the schools too exacting.[56]

There is no reason to believe that the physics textbook market is any different. This secret process is difficult to untangle, but as historian Robert Darnton argues, "By picking at the document where it is most opaque, we may be able to unravel an alien system of meaning."[57] Alvarez's textbook draft and the context surrounding it are as obscure as a lot of the choices made by physics textbook writers of the mid-century. That is not a deterrent, but a call to research.

STORYTELLING IN THE ALVAREZ TEXTBOOK

Alvarez's physics textbook was aimed at teaching graduate level experimental modern physics, a field that started with the discovery of X-rays in 1895. His approach was to include a lot of history, often reading like a history with equations. In particular, chapters three and six comprise a history of experimental modern physics broken into the

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[55] Fitzgerald, p. 22.
[56] *ibid*.
[57] Robert Darnton, *The Great Cat Massacre and Other Episodes in French Cultural History* (New York: Vintage Books, 1984), p. 5.

period before artificial radioactive bombardment and after. Other chapters vary in their historicity—the chapter on Einstein's relativity being particularly ahistorical—but these two chapters are mostly history and contain Alvarez's explanation for his approach.

Alvarez was writing exclusively about modern physics, so he knew enough of the players to be thoroughly historical. Besides justifying a historical approach, he first noted opposition to that style in his third chapter, titled "Radioactivity":

> There is a general feeling among teachers of nuclear physics that an historical treatment of the subject is not sound. Too many incorrect ideas must be developed which the student will later have to "unlearn." In addition, many concepts of late origin fit more logically into the general pattern treated along side some of the earliest developments. But one may easily go too far in eliminating historical references.[58]

Alvarez defended the historical approach, arguing that it helped students develop a deeper understanding, as if they were there figuring it out for themselves. His chapter three, covering 1895 to 1919, was an excellent example. Alvarez continued with his lengthy but quotable justification:

> One of the most important preparations a student needs if he is to do original work in any field is a thorough knowledge of what has been done by others before him. It is not sufficient merely to know the results of the important experiments and theories, which are reported in textbooks. One gains a real understanding of his field only when he is truly conversant with the detailed history of its development. He must know the working hypotheses which were used and discarded, the names of the men and women who made significant contributions, where they worked and why, why it was that certain experiments were done in one laboratory and not another, why other experiments were considered too preposterous to try until someone did them successfully, and most important of all, why someone happened to be doing the experiments which led to the really significant advances in the field.[59]

[58] Alvarez textbook manuscript, chapter 3, p. 1. LWAP, box 56, folder "Chapter III (Physics Text)."
[59] *ibid*.

So Alvarez defended the approach in terms of pedagogy, not in terms of curiosity that motivated Burns. Burns argued that the historical approach would help motivate and engage the student, but there was also a sense of fairness when he compared the paucity of literature on Faraday to that of political leaders: “Have you ever heard of Napoleon? Of course you have.”[60] Alvarez believed that physics could not be taught properly without learning from the techniques and mistakes of others. That is, historicity was a pleasant bonus for Burns that might help engage the student but an essential component for Alvarez who believed a physicist not aware of the past would not be a good experimentalist. Although Alvarez couched his approach in pedagogy, he did also allow for curiosity:

> The author has always been interested in learning why certain experiments were done by certain men, and will endeavor to pass this information on to the student at the appropriate times.[61]

The story in his textbook was personal for Alvarez, even when he did not know those involved.

Alvarez encouraged the student to pick up where he left off and read more about the lives of great physicists. It is standard practice in journals to include citations, but it is much less common in textbooks. Alvarez cited thoroughly, allowing the student to learn the physics the way he did: reading physics journals. He described his efforts to finish his Ph.D. at Chicago: “I had to learn physics theory to pass my doctoral oral examinations and had to do so from textbooks rather than from the several years of

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[60] Burns, *Physics: A Basic Science*, p. *v*.

[61] Alvarez textbook manuscript, chapter 3, p. 2. LWAP, box 56, folder “Chapter III (Physics Text).”

lectures I had neglected to attend."[62] However, he also described how poor that education had been: "By almost any standard, my training at Chicago had been atrocious. I had learned very little theoretical physics. My self-taught experimental skills were largely outmoded."[63] Alvarez cataloged his failings: "I knew almost nothing of nuclear physics, radioactivity, radio-frequency engineering, high-gain amplifiers, modern vacuum practice, or electrical engineering."[64] Instead, he learned physics through journals, both in their original form and in a compilation of three articles by Hans Bethe colloquially called "Bethe's Bible." "The only way that I could really learn nuclear physics, I concluded, was to read *everything* that had been written on the subject."[65] He meant everything:

> I carried home one annual volume after another of the old *Philosophical Magazine*, *Nature*, the *Proceedings of the Royal Society*, the *Proceedings of the Physical Society of London*, and for the years after nuclear-physics research caught on in the United States, the *Physical Review*. I supplemented my reading of the literature in English by examining summaries in *Science Abstracts* of French and German work. If I saw something interesting there, I looked up the full-length report.[66]

Alvarez's textbook allowed the student to learn as he had because it cited most relevant articles. He pushed some articles as if reading them in the original was essential. Roentgen's paper on X-rays earned high praise: "His first paper, which is a truly remarkable document, demonstrated a great many other properties of the radiation. This

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[62] Alvarez, *Adventures*, p. 38.
[63] *ibid.*, p. 45.
[64] *ibid.*
[65] *ibid.*, p. 47. Emphasis in original.
[66] *ibid.*

classic report is available in translation, and should be read by all students."[67] That paper marked the beginning of Alvarez's chapter three. Alvarez's retraining in the journal stacks started with Rutherford: "I arbitrarily decided that the beginning point of my reading should be Ernest Rutherford's 1919 disintegration of the nitrogen atom by alpha-particle bombardment, an event that changed nuclear physics from an observational science like astronomy to an interactive one."[68] That paper marked the beginning of Alvarez's chapter six.

Alvarez also included citations to history books. When he knew of a well-written history of physics, he would cite that and even offer praise. Often, he would cite because he wanted to give credit to a book or article that had guided his textbook's account of history. On the section on early radioactivity, he noted that "the author will follow closely the very excellent historical summary by the late Professor G. E. H. Jauncey."[69] Another time, he recommended an entire biography: "Much of the background information about the early work comes from Eve's biography of Rutherford."[70] However, he also stated that his account of the more recent history of modern experimental physics came not from his reading, but from his direct contact with physicists: "all of it concerning the later experiments derives from conversations with the

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[67] Alvarez textbook manuscript, chapter 3, pp. 2-3. LWAP, box 56, folder "Chapter III (Physics Text)." The Roentgen article is translated on page 600 of W.F. Magie, *Source Book in Physics* (New York: McGraw-Hill, 1935).
[68] Alvarez, *Adventures*, p. 47.
[69] Alvarez textbook manuscript, chapter 3, p. 2. LWAP, box 56, folder "Chapter III (Physics Text)." That summary was G. E. H. Jauncey, "The Early Days of Radioactivity," *American Journal of Physics* 14, no. 4 (1946), 226.
[70] Alvarez textbook manuscript, chapter 3, p. 2. LWAP, box 56, folder "Chapter III (Physics Text)." The Rutherford biography is Arthur Stuart Eve, *Rutherford; Being the Life and Letters of the Rt. Hon. Lord Rutherford, O. M.* (New York: Macmillan, 1939).

men responsible, or with colleagues of theirs who were present at the time."[71] Chapter three covered a period before Alvarez became a well-known physicist, but chapter six, though it started at Cambridge, ended with Alvarez's friends and associates.

Chapter three is followed by a chapter on early atomic models discussed below, a chapter on "Mass Spectra and Isotopes" which we will skip, and then chapter six on particle accelerators, which was Alvarez's specialty. More specifically, Alvarez worked on the particle detectors at the business end of particle accelerators, but accelerators were certainly in his field. His textbook had a second chapter on particle accelerators, but it is incomplete and not as interesting.

Chapter six, dated 18 February 1953, takes much the same approach as chapter three, basically walking through the history of particle accelerators with mathematical explanations of all the major issues that had to be resolved by the physicists who worked on them. The chapter begins with an experiment of Rutherford's mentioned above, that Alvarez said changed the nature of modern experimental physics. Rutherford shot alpha particles through nitrogen gas:

> From the discovery of radioactivity in 1896 until 1919, nuclear physics was essentially an observational science. Astronomy is such a science, in that one can only observe the stars as they are; no one is able to conduct tests to see how physical properties change as the result of applied forces. In 1919, Rutherford observed the first nuclear reaction actually under the control of an experimenter. Before that time, all observed nuclear reactions, such as α, β, and γ ray emissions, happened at a rate quite independent of the presence or absence of an experimentalist.[72]

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[71] *ibid*.

[72] Alvarez textbook manuscript, chapter 6, p. 1. LWAP, box 56, folder "Chapter VI (Physics Text)."

This experiment is arguably the precursor to modern particle accelerators in that particle accelerators do exactly that—shoot subatomic particles at targets to see what comes out. Alvarez moves on to more reliable ways of shooting alpha particles, from Van de Graff generators to the cyclotron that his mentor Ernest Lawrence invented to the larger machines on which Alvarez did his Nobel work.

Although Rutherford did what is arguably the first controlled experiment in modern nuclear physics, superior results came when physicists developed a better source of accelerated particles. "The first nuclear reaction to be induced by bombardment with 'artificially produced' high speed particles, was observed by Cockroft and Walton" in 1932.[73] This was a tremendous improvement over Rutherford's earlier work, which was based on waiting for alpha particles to be naturally emitted by radium and hoping that they would go where the experimenter wanted them to go. They were not steered toward a target, they were merely shot through slits that blocked particles that were headed in the wrong direction, as most of them were. Furthermore, Alvarez reported that a gram of radium cost about $75,000 in the 1920s.[74] John Cockroft and Ernest Walton advanced the field greatly by creating a reliable source of accelerated particles with a vacuum tube that had a voltage difference across two metal plates: the positive alpha particle would be repulsed by the positive plate and attracted to the negative one. Why not start the chapter with Cockroft and Walton's invention of the first rudimentary particle accelerator instead of Rutherford's experiment with naturally occurring particles?

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[73] *ibid*., p. 4.
[74] *ibid*., p. 2.

Perhaps we should note the end of the last sentence about Cockroft and Walton's experiment, which took place "at the Cavendish Laboratory of Cambridge University, of which Lord Rutherford was director."[75] Once again, Rutherford was part of the story, albeit in a supervisory role. The key here is that Alvarez idolized Rutherford. In his autobiography, Alvarez noted that Rutherford was his "number one hero."[76] Alvarez described his excitement when he first read "Rutherford's 1920 Bakerian Lecture, in which he first proposed the possibility of a neutron—an elementary particle not discovered for another twelve years,"[77] a credit historians do not usually grant to Rutherford because, arguably, a scientist should have some solid evidence to receive credit, not a hunch.[78] In writing in his autobiography of meeting the legendary pilot Chuck Yeager, Alvarez confessed that

> If you feel I'm dropping names, then much of this book will have made you feel that way, because I've recalled my experiences with friends who are, or were, well-known and world-class physicists. Heroes have been important to my development as a scientist.[79]

He lamented never having met Rutherford. Alvarez brought that hero worship to his textbook at a time when American physicists were only beginning to develop heroes, stories, and an identity.

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[75] *ibid*., p. 4.
[76] Alvarez, *Adventures*, p. 270.
[77] *ibid*., p. 47.
[78] For a discussion of who to credit for a discovery, see the debate over whether J. J. Thomson discovered the electron in Peter Achinstein, "Who Really Discovered the Electron?" in Jed Buchwald and Andrew Warwick, eds.,, *Histories of the Electron: The Birth of Microphysics* (Cambridge, Mass.: The MIT Press, 2001).
[79] Alvarez, *Adventures*, p. 270.

THE ALVAREZ TEXTBOOK ON SPECIAL RELATIVITY

Alvarez's first chapter, titled "Matter and Energy," is about Einstein's special theory of relativity, which is the more famous version. The general theory of relativity is about gravity and is complex enough that it usually gets its own graduate-level textbook and will not be discussed here. While the goal here is largely to illustrate Alvarez's early interest in historicizing physics for his readers, in his relativity chapter we see an uncharacteristically ahistorical approach. Perhaps Alvarez's chapter on relativity would have been more historical if Einstein had been an experimentalist instead of a theorist. After all, it was the story of experimentalists that Alvarez was trying to tell. Discussing this relatively ahistorical chapter is helpful to ensure a representative account of Alvarez's style that does not overplay his interest in history, but it is also useful as an example of instruction from a research physicist as compared to that from an educator. Surely the physics educator—whether a professional writer, a doctor of education, or a high school physics teacher with extensive experience teaching at the introductory level—would bring a helpful influence to the physics textbook, but this strategy often rests more on the author's writing ability than his or her grasp of the material. Harcourt Brace editor James Reid describes hiring Salem, Ohio, biology teacher Ella Thea Smith not for her accomplishments in the field, but because "the lady could *write*."[80] Her *Exploring Biology* became the second best selling biology textbook to date. In Alvarez's case, his connection to the practice of physics helps make the seemingly

[80] Reid, *An Adventure in Textbooks*, p. 69. Emphasis in original.

incomprehensible into a lucid account of one of modern physics' most famous enigmas, the special theory of relativity and Einstein's equation, $E=mc^2$.

Einstein's special theory of relativity of 1905 and its counterintuitive, even baffling, $E=mc^2$ equation often intimidate lay readers. That reputation was certainly earned by Einstein's 1915 general theory of relativity that posits a complexly curved space-time to explain gravity and cosmology. However, the basics of the Special Theory are within grasp of the average reader, especially if the mathematics is left out. It is a theory to explain the relationship between mass and energy, while clarifying the nature of light and time along the way. It is understandable if explained well.

Luis Alvarez's textbook draft of 1952 began with a chapter that makes parts of special relativity clear. Our purpose here is to discuss Alvarez's historical approach to physics pedagogy, but we will set the stage with a comparison between his textbook and a set of three textbooks from around the same time: Meyer 1944, Fuller 1948, and Brown 1954.[81] Here we take a strategy from Schick, who made his textbook analysis manageable by doing a broad but superficial survey of his large sample of physics textbooks and a thorough analysis of small subsamples on specific topics. This focused comparison with three very nearly contemporary physics textbooks will illuminate Alvarez's innovations in physics teaching.

There are two primary approaches to introducing special relativity. Many textbooks begin with Einstein's premise that the speed of light is the same for any

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[81] Jerome S. Meyer, *The ABC of Physics* (New York: The Dial Press, 1944); Robert W. Fuller, Raymond B. Brownlee, and D. Lee Baker, *Elements of Physics* (Boston: Allyn and Bacon, 1948); and H. Emmett Brown and Edward C. Schwachtgen, *Physics: The Story of Energy* (Boston: D.C. Heath and Co., 1954).

observer, regardless of the observer's velocity. Einstein thought that was the best interpretation of the laws of electromagnetism. He thought the alternative was unthinkable—what would a beam of light look like if one rode along side of it? The very idea of light waves standing still seemed absurd. These textbooks play out the consequences to show that a clock on a very fast-moving rocket slows down and the rocket becomes shorter as observed by a stationary observer. Of course, since no perspective is privileged, an observer on the rocket sees a stationary platform shortened and its clocks slowed. This is the only way to square the principle that the observer on the rocket and the stationary observer both measure the speed of a passing ray of light equally. This is a very common approach, the one used by Einstein himself in his popular account of his theory.[82] After some ingenious algebra, the theory can be shown to imply that the mass of the moving rocket will increase with its speed. It is as if its kinetic energy had a mass given by $m=E/c^2$ where m is the relativistic increase in mass, E is the rocket's kinetic energy, and c is the speed of light. That equation can easily be rearranged into the famous $E=mc^2$ equation that describes the equivalence of mass and energy. So one begins with a simple premise about the speed of light and ends with the equation that most people associate with Einstein.

Alvarez took the other approach, building on the law of the conservation of energy. During the Industrial Revolution in England, a great deal of study of steam engines led to the idea that energy cannot be created or destroyed. Heat from coal can

[82] Albert Einstein, *Relativity: the Special and the General Theory; a Popular Exposition* (London: Methuen, 1920).

become the motion of a train, which can become heat again in the friction of the train's axles and wheels. This concept was crucial to understanding efficiency, but it also puts a cap on efficiency: no system can create energy from nothing. There can be no perpetual motion machine. This is the founding principle of thermodynamics. From one point of view, Einstein's mass-energy equivalence equation shatters that fundamental principle. However, Alvarez's textbook pointed out that the law of the conservation of energy had already been called into question by radioactive radium first studied by Marie and Pierre Curie in the late nineteenth century. Alvarez explains, "Radium maintains itself at a temperature above its surroundings, in apparent violation of the laws of thermodynamics."[83] This realization came a few years before Einstein's special theory.

Having established that the conservation of energy was under attack from the 1890s, Alvarez reminds the student of another anomaly of Victorian physics—that light exerts a force on objects. Perhaps it should not be too surprising since light was believed to be a wave and other waves, like sound, exert pressure on objects. However, it is still a bit counterintuitive because light waves are not associated with any vibrating mass.[84] This radiation pressure is most often illustrated in textbooks by a radiometer, which is an evacuated glass bulb with four vanes painted black on one side and white on the other so the reflective side is pushed on harder by light, making the vanes spin when exposed to direct light.[85] James Clerk Maxwell, the father of electromagnetic theory, predicted this

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83 Alvarez, textbook manuscript, chapter 1, p. 1. LWAP, box 56, folder "Chapter I (Physics Text)."

84 Victorian physicists proposed that light was a wave in an invisible "ether" to avoid this difficulty.

85 Like many other supposed facts in textbooks, this is also wrong. While there is a force on the vanes, it may be too weak to make them spin. If the light bouncing off the white side were responsible for the

effect in 1871, so it was known well before Einstein. Alvarez shows that this simple fact about light is enough to derive the equivalence of mass and energy.

Alvarez began by asking us to picture a long box sitting on a frictionless surface.[86] At the left end of the box, a light source fires a very strong but brief pulse of light toward the right end. The light source would recoil a bit, pushing the box to the left. When the light hits the right end, it would exert an equal force in the other direction, bringing the box to a stop. Alvarez considered the analogy to a similar box with a cannon instead of a light source and, using the known equation for radiation pressure, he showed that either the light pulse has mass like the cannon ball or it is possible to propel a box without interacting with its surroundings. This second possibility violates Newton's law that every action has an equal and opposite reaction and, Alvarez tells us, was disproven by experiment. If the pulse has mass, some straightforward algebra reveals it to be given by the familiar $E=mc^2$. This sort of direct argument from an easily demonstrated property of light such as radiation pressure takes the student from uncontroversial and safe facts to the seemingly abstruse $E=mc^2$ equation in a very few steps. Introductory physics textbooks could take a hint from Alvarez.

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rotation of the vanes, the white side would move away from the light. Instead, the dark side of the vanes move away from the light because they heat up more and this heat exerts a small force on the little bit of air in the chamber.

[86] Alvarez, textbook manuscript, chapter 1, pp. 4-6. LWAP, box 56, folder "Chapter I (Physics Text)."

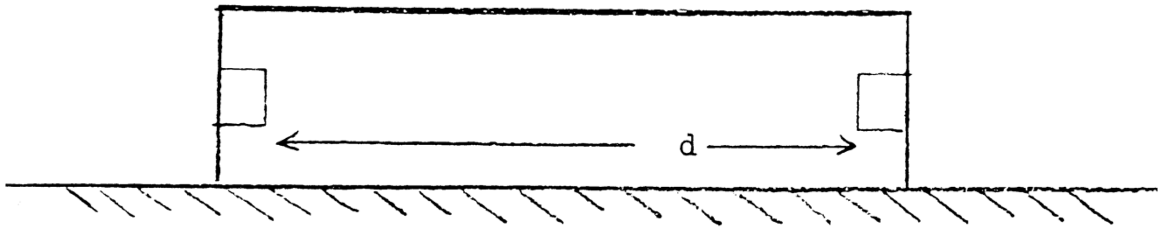

Figure 3.1: Alvarez described a box on a frictionless surface. The box has a length d. The small square on the left is a source of an intense and very brief pulse of light. The small square on the right absorbs that pulse. From Alvarez's chapter 1, page 5.

Of course, there is more to the special theory of relativity than mass-energy equivalence. Time dilation means that clocks on a fast-moving rocket slow down as its velocity approaches the speed of light. Length contraction means that a rocket gets shorter as it approaches the speed of light. These are two fairly easy to demonstrate results of the principle that light goes the same speed for any observer no matter how fast she is going. However, Alvarez managed to demystify the hardest to comprehend aspect of special relativity.

Obviously, Alvarez spends a lot more space on this than Meyer, Fuller, or Brown. However, it is not the depth of analysis but the pedagogical approach that interests us here. Elmer E. Burns' 1943 textbook, *Physics: A Basic Science*, did not cover relativity at all. It is not unusual for textbooks in this period to skip relativity, since it is counterintuitive enough to possibly confuse the young reader. Meyer's 1944 textbook spends two pages building up to Einstein, only to leave the theory essentially undefined. Fuller (1948) discussed mass-energy equivalence—Einstein's famous $E=mc^2$—as it relates to atomic bombs over the course of three pages, but Fuller wrote mainly in the context of the Manhattan Project, which is, arguably, an engineering problem, not a relativity problem.[87] Brown (1954) includes relativity in a section on the conservation of energy, as if Einstein upended this basic principle. Brown picks up again in a three-page section on atomic energy. Here, Brown follows in a similar fashion to Fuller, both carrying on the atomic enthusiasm of Cold War America and both making some mistakes in their physics.

The postwar press focused on the $E=mc^2$ equation and atomic bombs, most notably in a 1946 *Time Magazine* cover featuring Einstein's face in front of a mushroom cloud and an ominous, looming "$E=mc^2$" hovering in the cloud. However, all of special relativity can be boiled down to the simple realization that light treats all frames of reference equally. Light is a wave and waves need a medium—water waves, waves on a guitar string—and Victorian physicists called that medium "ether." That equality makes light unlike water waves since the water itself has a well-defined position; a person on a moving boat can observe waves spread faster towards the ship's stern. Of course, that realization leads to a few counterintuitive conclusions, but basically, if no frame of reference is to be privileged over any other, then it can be deduced that space and time must interact oddly and matter and energy must be alternate versions of the same stuff.

Let us begin with Meyer's textbook of 1944. This first edition textbook takes a fairly historical approach in that it gives the story of the "ether." This story has been told

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[87] Robert Serber makes a convincing argument for this in the introduction to his *The Los Alamos Primer* (Berkeley: University of California Press, 1992). This book is based on lectures Serber gave at Los Alamos in 1943.

many times and is basically about physicists trying to understand ether. Meyer took a historical approach to the story, explaining what physicists believed in the late nineteenth century and what they believed when his textbook went to press. The story of relativity is often described that way. The moment that physicists changed mindset seems very clear in human terms—Albert Einstein came along and swept away the old way of thinking. This is what Thomas Kuhn later called a "paradigm shift" and he used relativity as a classic example in his 1962 book, *The Structure of Scientific Revolutions*, along with Galileo's heroic struggle to show the Catholic Church that the earth goes around the sun.[88] Einstein and Galileo, along with Isaac Newton and Charles Darwin, exemplify scientific revolutions in the public mind in a way that captures the public's imagination, even if the actual science often eludes even the textbook writer. Meyer concludes the discussion of relativity with the confession that "The Theory of Relativity … is far too involved for us to consider here."[89] However, Meyer first briefly tells the story of the rise and fall of ether theory—a story much more concrete than the somewhat abstract relativity. Newton debated Huygens over the nature of light—is it waves or particles? Meyer introduced some of the historical characters: "Hertz discovered electromagnetic waves and Clerk Maxwell … produced the magnificent Electromagnetic Theory."[90] Meyer gave Huygens a birthdate and names of his major works; Hertz remained only a surname, even in the index.

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[88] Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: The University of Chicago Press, 1962).
[89] Meyer, *The ABC of Physics*, p. 176.
[90] *ibid.*, p. 175.

Meyer continues the story with the famous 1887 experiment by Albert Michelson and Edward Morley designed to study the ether. Surely, as the earth flies through space on its way around the sun and spins on its axis, the ether would appear to move past the surface of the earth. Michelson and Morley tried to detect the ether rushing through their device by measuring the speed of light in different directions. An observer on a boat could measure the speed of water waves in various directions and see that the waves moving toward the ship's bow move slower than those moving toward the stern. In a result that might have given Galileo pause, Michelson and Morley found no motion in the ether: light travels at precisely the same speed in all directions. Meyer reported this finding as "there is no ether as Huygens conceived it"[91] without any details about the experiment itself. This is, of course, the universal interpretation; physicists could not return to an earth-centered universe.

Where Meyer's story gets strange is the logical leap that gets us to relativity. He essentially dropped the Michelson-Morley experiment and pointed out that "light is actually a form of energy,"[92] an uncontroversial claim by any measure. However, he quickly descended into nonsense:

> Now we have already seen that kinetic energy is equal to half the mass times the square of its velocity; in other words, velocity has no meaning separated from mass. You cannot have motion without some mass moving. If, therefore, light is energy it must inevitably have some mass; and if it has mass, by the law of gravitation, it will be attracted in a gravitational field.[93]

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[91] *ibid*., pp. 175-6.
[92] *ibid*., p. 176.
[93] *ibid*.

He was referring to the equation for kinetic energy ($K.E.=\frac{1}{2}mv^2$) but he is clearly misguided in its interpretation. Kinetic energy is one type of energy, not the only type. Kinetic energy obviously requires mass and motion, but there is also potential energy. Whether potential energy is the chemical energy in gasoline, in a cocked spring, or simply the potential energy of a heavy weight suspended off the ground, it is not the case that energy is necessarily motion. A beam of light does have energy and does bend in a gravitational field, but Meyer's description is some convoluted hand waving.

These are the times that Meyer's casual approach is most noticeable. In the preface, Charles E. Falk, B.A., teaching fellow in physics at New York University's Washington Square College says, "Mr. Meyer is a layman who wrote the book for laymen in the language, not of the physicist, but of the layman."[94] This is meant as a compliment but the misconceptions in the text call into question the value of that approach. Many textbooks in this period include the author's credentials or job title; *ABC of Physics* does not. Fitzgerald points to the textbook industry for these types of flaws, recounting the story of a young editor with no credentials in education or teaching who, "with little help," wrote a book of short biographies; it was "published under the name of an elementary-school teacher."[95] She argues that the profitability of textbook publishers means that there is "no real check on the intellectual quality—or even the

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[94] *ibid.*, preface.
[95] Fitzgerald, *America Revised*, p. 26.

factual accuracy—of school textbooks."[96] Surely physics can be made interesting and intelligible without losing accuracy.

The 1948 textbook *Elements of Physics* by Robert W. Fuller, Raymond B. Brownlee, and D. Lee Baker keeps the wave-particle controversy in another section separate from Einstein's theory of relativity. Newton and Huygens get full names as well as birth and death dates while lesser-known physicists like Young and Fresnel are referred to by surname only. This line of reasoning does not end with Michelson and Morley and relativity, instead becoming a lead-in to the origins of the quantum theory. Instead, Fuller *et. al.* discuss Einstein solely in the context of atomic bombs:

> Up to this point we have considered mass and energy as entirely different things. But it has been shown that an exceedingly rapidly moving body has greater mass than the same body at rest. In all radioactive or other nuclear disintegration the total mass of the fragments is less than that of the disintegrating atom. Einstein showed that the energy produced is equal to the mass lost multiplied by the square of the velocity of light. Mass really represents a highly concentrated form of energy.[97]

In essence, Fuller only briefly mentioned Einstein and not in the context of the nature of light, but to summarize the $E=mc^2$ concept. To be sure, the nature of light and the equivalence of matter and energy are connected, but not in a way that is explained in elementary physics textbooks. Fuller did not explain why a fast-moving object is heavier than a stationary one, although recent physics textbooks do this well before getting to $E=mc^2$.

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[96] *ibid*., p. 43.
[97] Fuller, *Elements of Physics*, p. 758.

In 1954, New York State College for Teachers physics professor Henry Emmett Brown and high school science consultant Edward C. Schwachten published *Physics: The Story of Energy* through D.C. Heath and Company. In sections on atomic power, Brown, *et. al*. elaborate on the concept of matter turning into energy by plugging a bunch of numbers into Einstein's $E=mc^2$ equation. The speed of light is, in our usual units, very great, and obviously the square of its velocity would turn the tiniest amount of matter into enormous amounts of energy. In an atomic bomb, one pound of uranium only converts one tenth of a percent of its mass into energy yet provides the equivalent of the energy in 300,000 gallons of gasoline.[98] It is helpful here to note that such extremely large numbers come partly from our choice of units. If we simply define our unit of length as the distance light travels in one second, then the speed of light squared would equal precisely one; mass and energy would be equal in number. Something similar happens in sub-atomic physics where the electron volt is used both as a measure of mass and of energy. However, Brown was trying to impress his readers with big numbers. He points out that the sun converts four million tons of matter into energy each second.[99] That is impressive!

Brown adds to the understanding of relativity its effect on the previously explained concepts of the conservation of mass and conservation of energy. Here Brown introduced the historical Einstein twice early in the textbook before getting into modern physics. In a section on steam and gasoline engines, he introduced the idea of the

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[98] Brown, *Physics: The Story of Energy*, p. 396.
[99] *ibid*., p. 553.

conservation of energy. That is appropriate historically since the concept emerged through the study of steam engines in Industrial Revolution-era England. The principle states simply that energy cannot be created or destroyed, only changed in form. Brown adds an allusion to $E=mc^2$ to this discussion of early-nineteenth century physics,

> Einstein, perhaps the greatest scientist of our times, in his work on relativity and on the relation between gravitational forces and motion, has surely been working along these lines.[100]

Brown then described the conservation of energy after having set the stage with a one paragraph gloss of work by Rumford, Davy, and Joule, all merely surnames in the great men of science tradition. He returned to Einstein:

> Until 1930 this principle was accepted as final, but the work of Einstein and the development of the atomic bomb indicate that matter may be changed into energy.[101]

As noted above, it would be more accurate to say Einstein turned the principles of the conservation of energy and the conservation of mass into a grander principle of the conservation of mass-energy, not that he weakened the earlier twin principles. The comment on nineteenth century science seems more like a wink to the student of modern physics that the scientists working a century before Einstein could not have known what we know now.

While Alvarez's coverage of the theory of relativity is less historical than that of Meyer, Fuller, and Brown, it is instructive nonetheless in that it exposes some of the problems with writing about physics for a lay audience. The elite physicist does not

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[100] *ibid*., p. 247.

[101] *ibid*. It is not clear why he chose 1930. Einstein published his special theory of relativity in 1905, fission was discovered in 1938, and the atomic bomb became known to the public in 1945.

usually write introductory physics textbooks and many publishers believe that that is a virtue; cannot the layperson write best for the lay audience? In the period after the Second World War, physics was more widely taught and that meant relating to a broader audience. Alvarez's approach is also instructive in that it exposes his goals. The operator of a particle accelerator does not need to know that a moving rocket gets shorter as it approaches the speed of light. He does need to know that a particle has more momentum and will need even more energy to be accelerated further. Alvarez has a very specific audience in mind. He is not writing for future theorists who might want to know how Einstein developed the ideas in special relativity. He is writing for future experimentalists specializing in the type of work on which Alvarez spent most of his early career. Alvarez's perspective in writing this textbook reappears in his chapter four on early models of the atom.

J. J. THOMSON AND THE PLUM PUDDING MODEL OF THE ATOM

The fourth chapter in Alvarez's textbook focuses on one episode of early atomic physics: Ernest Rutherford's successful overthrow of his old professor's atomic model. Alvarez spent an entire chapter examining one experiment that Rutherford published in 1911 on bombarding a gold foil with alpha particles. Along with relativity, this is the most extensive coverage afforded a single episode in his textbook. As noted above, Alvarez called Rutherford his "number one hero" and perhaps that is why this experiment got so much attention. Arguably, Niels Bohr's atomic model of only two years later was of far greater consequence to the development of physics. Why should Rutherford's model, which was the dominant model for only two years, be of such significance?

Thomson's model reigned for over a decade, yet he comes off as a foil to Rutherford. Besides being Alvarez's hero, Rutherford was an experimentalist, while Bohr was a theorist and the textbook was meant to train experimentalists.

There is some sleight of hand needed to make Rutherford the hero and Thomson the simplistic precursor. In 1897 and more extensively in 1904, J. J. Thomson developed an early model of the atom now generally referred to as the "plum pudding" model.[102] The model essentially said that atoms are composed of negative electrons—or corpuscles as he called them—and their negative charge was balanced by some unknown source of positive charge, which he initially described as a "sphere filled with uniform positive electrification,"[103] which he borrowed from Lord Kelvin. In a speech in November 1901 and subsequent paper in 1902, Kelvin proposed a preliminary model of the atom.[104] He argued that the sphere of positive charge was the simplest explanation and should be a working model until there was more experimental data. Thomson picked up on this model and extended it into an elaborate system of electrons orbiting in rings on parallel planes in his full model of 1904.[105] That model became the dominant atomic model for a number of years, despite its lack of verified predictions. He tried to explain the

102 For a fuller discussion of this model and its development, see Per F. Dahl, *Flash of the Cathode Rays: A History of J. J. Thomson's Electron* (Bristol: Institute of Physics Publishing, 1997) and J. Rubén Martínez, "Plum Pudding and the Folklore of Physics," unpublished manuscript. Thomson's article describing the discovery of the electron included a brief sketch of what would be fleshed out in his 1904 model without specifying what force held the electrons together or what positive charge balanced their negative charge. J. J. Thomson, "On Cathode Rays," *Philosophical Magazine* ser. 5, vol. 44 (Oct. 1897), p. 311.

103 Thomson, "The Magnetic Properties of Systems of Corpuscles describing Circular Orbits," *Philosophical Magazine* ser. 6, vol. 6 (Dec. 1903), p. 673.

104 Lord Kelvin, "Aepinus Atomized," *Philosophical Magazine* ser. 6, vol. 3 (Mar. 1902), p. 257.

105 J. J. Thomson, "On the Structure of the Atom: an Investigation of the Stability and Periods of Oscillations of a number of Corpuscles arranged at equal intervals around the Circumference of a Circle; with Application of the results to the Theory of Atomic Structure" *Philosophical Magazine* ser. 6, vol. 7 (Mar 1904), pp. 237-265.

periodicity of Mendeleev's periodic table, radiation, and eventually even the photoelectric effect, although the data did little to support his claims.[106] Oddly, the model described in textbooks today most closely resembles Kelvin's model, but textbooks and popularizations after the 1960s refer to it as Thomson's plum pudding.

Thomson's contributions to early atomic models are often derided in histories and physics textbooks after the late 1960s as simplistic, an argument that we will refute. The plum pudding depicted in textbooks today is simpler and implies characteristics that were not part of Thomson's model. For one, images in textbooks seem to imply that the electrons are immobile, while Thomson's model was dynamic. This seems to derive from the mental image one gets from plum pudding: a solid bread with raisins stuck in it. Plum pudding is an English Christmas dessert with dried fruit, especially raisins, mixed into a dense, bready cake. The expression "like plums in a pudding" was a common expression beginning in nineteenth century England, meaning grains or lumps spread throughout some other material.[107] It should be noted that there are no plums in plum pudding.[108]

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[106] Thomson's efforts to explain the photoelectric effect, as well as Roentgen rays, now known as X-rays, came in a paper published after Rutherford and Bohr had moved on to a modern atomic model. Thomson, "On the Structure of the Atom," *Philosophical Magazine* ser. 6, vol. 26, p. 793.

[107] The expression became commonly used soon after Charles Dickens used it in "A Morning Call on a Great Personage," in *Household Words* (New York: Bradbury and Evens, 1850), p. 6.

[108] Auguste Escoffier, *Le Guide Culinaire*, translated as *The Escoffier Cookbook* (New York: Crown Publishers, Inc., 1989), p.764. Escoffier, although French, is a tremendously influential chef and creator of the *brigade de cuisine* system. *Le Guide Culinaire* was originally published in 1903.

In 1911, Rutherford, with the help of his students Hans Geiger and Ernest Marsden, shot alpha particles at a leaf of gold foil.[109] They were studying the deflection of the particles when Rutherford suggested they check for deflections of more than ninety degrees, in other words, that they check to see if any particles bounced back. It turned out that a very small fraction did bounce back. That was unexpected under Thomson's model. Alvarez may have been impressed with Rutherford's instinct as an experimentalist even to think to look where the current paradigm said there should be nothing. The result led Rutherford to infer that the positive charge was not contained in a diffuse sphere the size of the atom but in a concentrated nucleus with enough mass to deflect alpha particles.

Physicists defend the popular version of this story because of its pedagogical value in explaining how physics works. It has become a classic tale of experimental instinct comparable to Robert Millikan's ability to throw out bad data when measuring the charge on the electron in his oil drop experiment. Thomson became a straw man in the story of Rutherford's clever experiment. Thomson was polarizing as head of the Cavendish Laboratory at Cambridge. To cite just one example, Cambridge physicist Joseph Larmor wrote to Rutherford:

> I am glad you are seeking an understanding with [Thomson] but your position in no case depends on it. He was always an isolated figure here [at Cambridge], had very little contact with mathematicians or chemists who

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[109] Ernest Rutherford, "The Scattering of α and β Particles by Matter and the Structure of the Atom." *Philosophical Magazine* ser. 6, vol. 21 (May): 669.

would have responded to contact. … They complain of J. J. T. that he not only did not give a lead, but poured cold water over projects.[110]

Physicist Abraham Pais certainly portrayed Thomson as pathetic and slow to adapt to new theories.[111] This unpopularity could have helped make physicists critical of his atomic model and more likely to make a hero of Rutherford. Thomson's model, while wrong, was distilled into a simplistic caricature that leaves the young student wondering how a Nobel Prize winning physicist like Thomson could believe atoms were pudding-like blobs with no real structure. Alvarez certainly seemed interested in promoting that version of this story, one reshaped to highlight his hero Rutherford.

Fig. C. Diagram of the Thomson atom model.

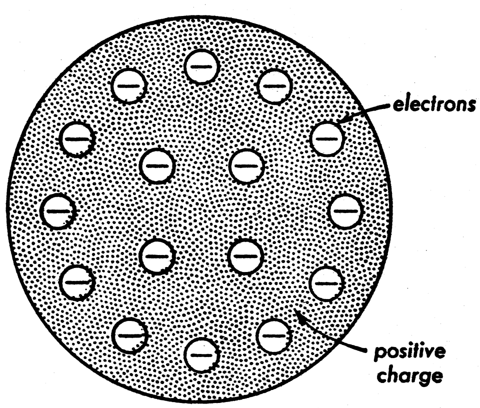

Figure 3.2: This 1959 image of plum pudding is the first to appear in print. Notably, it is nearly accurate to Thomson's 1904 model. While the text does not mention the arrangement of electrons, its ring configuration portrayed here approximates Thomson's proposal, although the specific arrangement is not one of Thomson's allowable arrangements. From Harvey Elliott White, *Physics: An Exact Science* (Princeton: D. Van Nostrand, 1959).

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[110] Joseph Larmor to Ernest Rutherford, 9 Mar. 1919. The Papers of Ernest, Lord Rutherford. Cambridge University Library, Add.MS.7653.

[111] Abraham Pais, *Inward Bound: Of Matter and Forces in the Physical World* (Oxford: Clarendon Press, 1986), p. 187.

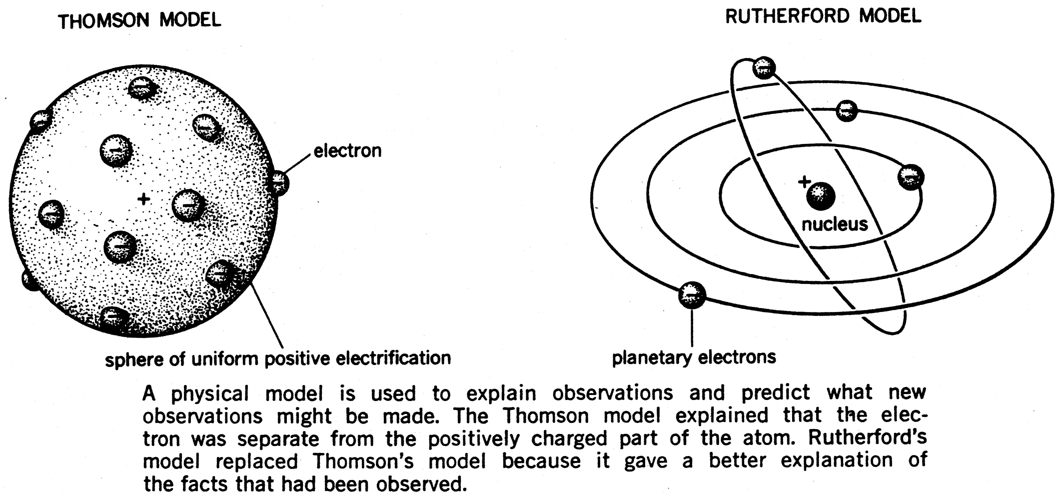

Figure 3.3: By 1967, images of plum pudding had become common. Note the interpretation of "embedded" electrons is rendered as fixed in what appears to be a solid mass of positive charge. The electrons seem to be randomly scattered on the surface. This is substantially different than Thomson's ring arrangement. This image is from W. B. Herron, *Matter, Life, and Energy* (Chicago: Lyons and Carnahan, 1967).

But why study bad history? Good history is harder to trace. If a textbook writer makes an accurate claim about the atom, it can be unclear where the claim originated. He or she may have read the original article, read it in another textbook, or heard it second or third-hand. We cannot trace the provenance of his assertion. However, a false claim is often traceable if it is more elaborate than a simple mix-up or malapropism. Falsehoods as specific as Thomson's authorship of the plum pudding model originated with one person and are unlikely to have had multiple origins and, being unnecessary to the teaching of physics, are taught as a conspicuous act. What we are after is not the structure of the atom, but the mechanisms by which physics is communicated and the motivations behind that transfer of knowledge. It is part of teaching, which is, in essence, a form of recruitment. The understanding of any social group, any identity, must appreciate the relationship with the other that is explored in chapter five and the relationship with that part of the other that the community wants to recruit, the physics student.

Alvarez's approach to the plum pudding model reveals his perspective and his goals—he is an experimentalist trying to teach experimentalists. His draft of chapter four, dated 17 December 1952, uses the historical physics as an opportunity to work through the achievements of physics greats like J. J. Thomson and Ernest Rutherford. Alvarez was clearly an insider, familiar with both the literature and the lore of physics. He did not use the phrase "plum pudding," but alternated between a close reading of Thomson and Rutherford's original articles and the version of the story as interpreted through physicists' mythology. Curiously, he often misses the disconnect while sometimes catching it only in handwritten editorial comments. He does not seem to have caught on to the fallacy of the myth.

Alvarez started the chapter by noting that "Until 1911, when Rutherford proved the existence of the atomic nucleus, atoms were generally considered to be small spherical objects with approximately uniform density."[112] This is a common refrain in physics textbooks and, like the others surveyed here, Alvarez does not give a source. It is plausible that physicists developed that assumption in retrospect, since early atomic theorists like John Dalton do not seem to have worried about the structure of atoms. The whole point of an atom—Greek for "indivisible"—was that it is the smallest possible

[112] Alvarez textbook manuscript, chapter 4, p. 1. LWAP, box 56, folder "Chapter IV (Physics Text)."

particle, not the next step in a miniaturization progression. After all, Alvarez noted that physicists understood that the theoretical limits of microscopes would never allow a look inside an atom. This introductory sentence is oddly dismissive of Thomson's model of 1904, which was not merely a ball of "nearly uniform density," but it is a common wording found in physics textbooks.

Thomson's model serves as backdrop to the work of Ernest Rutherford and his students Hans Geiger and Ernest Marsden. Alvarez described Thomson's model:

> Of the various atomic models proposed before 1910, that of J. J. Thomson received the greatest support. He pictured the atom as a sphere of positive electricity in which negative electrons were embedded. The electrons were arranged in concentric spherical shells, which rotated as units.[113]

This description is about right—substantially more correct than most textbook accounts—and reflects that Alvarez had probably read Thomson's 1904 paper. Further evidence is that he correctly noted that "The properties of an atom were believed by Thomson to derive from the particular arrangement of electrons in their shells."[114] Some other textbooks note this, but few note that "The nature of the positive charge was not specified; it could be a continuous distribution, or could consist of discrete charged particles."[115] The inclusion of this fact, from a Thomson paper in 1910,[116] indicates that Alvarez was very familiar with Thomson's original articles. Alvarez claimed to have an excellent recall for physics articles:

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[113] *ibid*.
[114] *ibid*., p. 2.
[115] *ibid*.
[116] Thomson, "On the Scattering of Rapidly Moving Electrified Particles," *Cambridge Lit. & Phil. Society*, 15, pt. 5 (1910), pp. 465-466.

> I can't reproduce equations or text from memory, but I can often remember author's names, which journal, and even the precise location of an important graph in an article read rapidly years before. My memory has come to be so extensively cross-referenced that I can walk into a library and find in a minute or two almost any article I've ever read.[117]

This reading of Thomson's model allows that it was in fact compatible with Rutherford's discovery of the nucleus. After all, if Rutherford found that the atom had a dense, massive nucleus, could that not be one large "discretely charged" particle of the kind that Thomson allowed? Rutherford was certainly aware of Thomson's 1910 paper allowing discrete particles of positive electricity; Rutherford's 1911 paper announcing the discovery of the nucleus cited Thomson's 1910 paper. This parallels the story of Rutherford's quick-witted effort to save himself from the famous wrath of an elder scientist. In 1904, Rutherford gave a lecture that would contradict Kelvin. When he spotted Kelvin in the audience, he realized

> that I was in for trouble at the last part of my speech dealing with the age of the earth, where my views conflicted with his. To my relief, Kelvin fell fast asleep, but as I came to the important point, I saw the old bird sit up, open an eye and cock a baleful glance at me! Then a sudden inspiration came, and I said Lord Kelvin had limited the age of the earth, *provided no new source was discovered*. That prophetic utterance refers to what we are now considering tonight, radium! Behold! the old boy beamed upon me.[118]

Apparently, Rutherford had more respect for Lord Kelvin than for Thomson, or maybe he feared Kelvin's wrath more than Thomson's. Perhaps he would have given Thomson the same credit under similar circumstances, but it does seem odd that he was not as forthcoming in print about Thomson's "discretely charged" positive particles. The

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[117] Alvarez, *Adventures*. p. 22.

[118] Bruce Hunt, *Pursuing Power and Light: Technology and Physics from James Watt to Albert Einstein* (Baltimore: Johns Hopkins University Press, 2010), pp. 44-45.

possibility that Rutherford expanded on Thomson's model, not destroyed it, has been overlooked by other physicists and historians.

In his draft textbook, Alvarez usually cited original articles, for example, Rutherford's 1906 article on photographic detection of α-particles or the copious citations in his chapter three. In chapter four on atomic models, he often commented in handwriting "ref?" including several times on page eight when discussing Geiger's work on Geiger counters. Perhaps this chapter is in a less finished state. He did not cite Thomson's papers at all. This suggests that he was very familiar with them, but did not have them on hand as he wrote. This might have changed in later drafts if he had finished this textbook.

In Alvarez's description of Thomson's model, he, underlined "rotated" or "rotating" a total of three times, adding a handwritten "why rotation?" Thomson's paper of 1904 explains that "The rotation is required to make the arrangement stable when the [electrons] can move at right angles to the plane of the ring."[119] In other words, he was trying to get the electrons to arrange themselves in rings that he would use to predict the chemical and physical properties of the atom. This project was ultimately not successful. The laws of electricity and magnetism say orbiting charged electrons should radiate light or x-rays. Alvarez tried to get into Thomson's head by explaining that a shell embedded with electrons could form something similar to "A uniformly electrified, rotating sphere" which "does not radiate according to classical theory."[120] Perhaps Alvarez's "why

[119] Thomson, "On the Structure of the Atom," p. 255.

[120] Alvarez textbook manuscript, chapter 4, p. 1. LWAP, box 56, folder "Chapter IV (Physics Text)."

rotation?" was an attempt to correct a flaw in Thomson's model. This is often cited as the major flaw in Thomson's model and Rutherford's model is often described as having solved the problems in the Thomson model. This is not the proper venue to pursue that point.[121] Suffice it to say that the standard story says that Thomson's 1904 model was wrong because it should radiate and real atoms do not normally radiate. Then, the standard story goes, Rutherford came along in 1911 and showed that atoms have a nucleus around which electrons orbit, proving Thomson wrong. Then in 1913 Niels Bohr showed why orbiting electrons do not radiate by applying the new quantum mechanics to the atomic model. (The answer, in lay terms, is that electrons can only radiate in discrete quanta of radiation—whole photons, not partial photons. Electrons in stable atoms have already given off any whole quanta of energy available.) The standard story does not point out that the supposed wrongness of the Thomson model—that it should radiate—is also a flaw of the Rutherford model. Alvarez did note that Thomson allowed for a positive charge concentrated in "discrete charged particles," and presumably in one such particle, but the entirety of his chapter four tells a story of a wrong-headed Thomson corrected by a brilliant Rutherford.

Getting the history wrong works for Alvarez's purposes—he wanted to teach young experimentalists to process the type of statistical data Rutherford, Geiger, and Marsden were getting. They were shooting α-particles at thin foils of gold and studying the angle of deflection. Alvarez worked through the mathematics of this deflection and

[121] Again, see Martínez, "Plum Pudding and the Folklore of Physics" for a much more detailed history of plum pudding and the unfair way that Thomson has been characterized in physics textbooks and popularizations.

here we see his goal—teaching students how to interpret statistical data. He did, however, work from modern assumptions. The graduate physics student does not need to work from such antiquated assumptions as Thomson or Rutherford used. On page eleven of this chapter, Alvarez began a thorough derivation of Rutherford's conclusions, but did so with assumptions that were not available to Rutherford. Alvarez wanted his students to put themselves in Rutherford's shoes without losing sight of the knowledge of modern physics, so he assumed that:

> 1.) The α-particle has a positive charge equal to 2e.
>
> 2.) The nucleus has a positive charge Ze, and a mass so great compared to that of the α-particle that its recoil during the scattering process may be neglected. …
>
> 3.) The kinetic energy of the alpha particle is very small compared to its rest energy, so the non-relativistic equations may be employed.[122]

He explained that "The derivation given below seems to the author to emphasize more clearly the real physics of the problem."[123] Alvarez wanted the student to understand the historical problem in order to relate to a new problem like one they might encounter in their own work. He does not want to impress upon the student any false physics, even though Rutherford did not know those assumptions, except perhaps the third.

When looking at textbook histories of the Thomson and Rutherford atomic models, patterns emerge that reveal how the physicists' mythology evolved. Certain phrases, ideas, and even images pop up again and again in a way that allows us to trace

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[122] Alvarez textbook manuscript, chapter 4, p. 11. LWAP, box 56, folder "Chapter IV (Physics Text)." "e" is the charge of one electron, Z is an arbitrary integer. The third assumption should have been available to Rutherford, but it would have been of little consequence to his experiment.
[123] *ibid*.

the origins of certain pieces of the story. We seek these patterns not just for the sake of curiosity, but because this type of analysis tells us a great deal about where physics lore comes from—what are the sources for the stories physicists tell about themselves? The plum pudding story is a valuable key in that the truth has a clear source, but fictions are made up by actors with some other motivation and they are easier to trace because they generally have a single origin.

Much of this tradition is oral. As Richard Rhodes noted,

> One of the really deep truths about the physics and other science communities [is that] they really are almost apprentice based, guild based in their tradition of learning, not simply formal and academic. There are so many tricks of the trade, if you will, that only can be learned by hands-on experience. And that is even true I'm sure of theoretical physics but God knows it is true of experimental physics where you never get the same result twice.[124]

Alvarez, writing in his textbook chapter on particle accelerators, agreed:

> Some of these pieces of information are of course available in the scientific literature, but too many are learned only through informal conversations with men who have spent years with the subject. Such men have a great store of background material which never seems to be important enough to publish, but which makes a great deal of difference in their approach to new problems.[125]

Even graduate students, well versed in the literature and fresh from textbook-heavy coursework, have to discover this unpublished knowledge orally:

> Graduate students are always proposing experiments to their research professors who have to say, "That is a fine idea, but it has been tried three times by men I know, all of whom got negative results which they didn't feel justified publication."[126]

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124 Rhodes interview by author, 20 May 2009, p. 28.
125 Alvarez textbook manuscript, chapter 6, p. 1. LWAP, box 56, folder "Chapter VI (Physics Text)."
126 *ibid*., pp. 1-2.

Alvarez described one way that physicists at Berkeley communicated. Ernest Lawrence ran a weekly "Journal Club" in addition to whatever other interactions physicists had:

> The interaction was strong. [Theorists] Robert Oppenheimer, Bob Serber, Bob Christy, people of that sort talked with the experimental people frequently on an almost day-to-day basis. Everyone came to the Journal Club on Monday nights; that was something nobody missed. No wives would think of scheduling anything on Monday night.[127]

The trick to making the Journal Club work was keeping the speaker secret.

> The speaker could be a graduate student giving his first public report or Robert Oppenheimer, the distinguished American theoretical physicist, announcing his theoretical discovery of neutron stars.[128]

The club fell apart as the secrecy rule was loosened. Alvarez ran a similar club at Berkeley for twenty-seven years, making sure even graduate students could hear cutting-edge research. The historian has little access to this oral tradition.

Oral interviews of physicists still alive can be very helpful, but Luis Alvarez passed away in 1988. Our goal here to identify the early strains of identity formation in American physics would require interviews with physicists working in the years before the Manhattan Project. Alvarez was interviewed many times, most notably by Charles Weiner and Barry Richman in 1967, but even if Weiner and Richman had asked him and others where this plum pudding story came from, they would likely say that it is what they heard in college or graduate school but would probably not know the story's provenance. Every time physicists tell the story, they do so with a purpose different from the historian's goal of accurate storytelling.

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[127] Alvarez interview by Charles Weiner and Barry Richman 14 Feb. 1967, session I, p. 32.
[128] Alvarez, *Adventures*, p. 47.

As we have seen, Alvarez's goal was to prepare his graduate students for the kinds of problems experimental physicists have to deal with. Pedagogy is one of the primary goals in textbooks, but perhaps not the only one, as Rachel Hinckley demonstrated in her dissertation on the cultural implications of mathematics textbooks.[129] She showed that mathematics texts—seemingly one of the least value-laden types of textbooks—carried messages of morality and nationalism, primarily in the word problems. The plum pudding story as it appears in recent textbooks came together piecemeal from some original facts. The key to understanding the story is to identify these varied sources and the provenance of the plum pudding tales.

Physicists communicate this story largely through physics textbooks. Textbooks are, of course, one of the primary ways the physics community recruits new members, in addition to the oral tradition discussed above. When Alvarez described the Thomson model of the atom as having "a sphere of positive electricity in which negatives electrons were embedded," he was using nearly exactly the same language as a large number of textbooks, but not the language of Thomson. We can read what Thomson wrote in physics journals in 1897, 1899, 1903, and especially in 1904 when he laid out the most fully developed version of his atomic model. What we find is that "embedded" is not an apt description of his dynamic model. "Embedded" implies a heavy, solid material for electrons to be embedded in. "Embedded" sounds like the heavy, sweet, English bread desert called plum pudding with its raisins stuck in it. As far as a search through the published literature, Thomson's autobiography, and the J. J. Thomson papers at

[129] Hinckley, "American Culture as Reflected in Mathematical Schoolbooks."

Cambridge University suggests, Thomson never described his model as resembling a "plum pudding."[130] So where did "plum pudding" with "embedded" electrons come from?

James Arnold Crowther was the first to use the phrase "like plums in a pudding" in print to describe the Thomson model in 1923.[131] Crowther was a demonstrator at the Cavendish and a lecturer at Cambridge, so he must have known Thomson. In his introduction, he commented on the development of the electron theory: "The foundations of this part of the subject were well and truly laid by the genius of Sir J. J. Thomson and his fellow workers, and are not likely to he overturned."[132] Like White, who benefited from his proximity to Lawrence, Alvarez and others, Crowther knew many of the men who would appear in his textbook. To say that his text diminishes the historicity of White and Burns is to forget that Crowther was English; American physics developed very differently. In 1952, Sir John Cockroft, also of the Cavendish Laboratory, noted in his Rutherford Memorial Lecture that Rutherford said once that Thomson thought "the atom was like a plum pudding."[133] It seems likely that Rutherford, working on atomic models after Thomson, came up with the term. It may seem like a slight, but Rutherford was known to use homespun expressions in physics. However, the term did end up shaping future physicists' understanding of the model, simplifying it and making a hero

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130 His autobiography is J. J. Thomson, *Recollections and Reflections*. (New York: The MacMillan Company, 1937). His papers are "Papers and correspondence of Sir Joseph John Thomson, 1856-1940," held at the Trinity College Library, University of Cambridge, Add.MS.7654. His papers are only six boxes and were not terribly helpful.

131 James Arnold Crowther, *Molecular Physics* (Philadelphia: P. Blakiston's Son & Co., 1923), p. 74.

132 *ibid*., p. *v*.

133 John Cockroft, "The Rutherford memorial Lecture," *Proceedings of the Royal Society of London. Series A, Mathematical and Physical Sciences*. Vol. 217, No. 1128 (Mar., 1953), p. 2. Delivered 20 Sep. 1952.

of Rutherford. The historical record has given us little more than these hints to find the origins of "plum pudding."

Wherever plum pudding comes from, we know that physics lore can be transmitted and added to through textbooks, oral tradition, physics journals, popularizations, histories, and perhaps other sources. Perhaps movies about physicists will become Hollywood's next fad, or perhaps it will be television sitcoms such as "The Big Bang Theory." Some of these sources, like journals, popularizations, and histories, are the regular stuff of historical research. Textbooks are somewhat overlooked and it is one goal of this chapter to help remedy that. The oral tradition can be opaque particularly for an era whose actors are no longer with us. How should the historian of physics proceed?

The historian of science may borrow a technique from theology. In the early twentieth century, theologians developed a theory that the common passages in the gospels of Mark, Matthew, and Luke revealed a pattern.[134] They proposed that Matthew and Luke had both read Mark, a theory known as "Markan priority." They further proposed a secondary source of Jesus' sayings that they called "Q," short for "quelle," German for "source." This "two-source theory," as theologians call it, has gained wide appeal. It allows theologians to deduce what an unseen source contains from commonalities in Matthew and Luke that are absent in Mark. The historian of physics could do as well in identifying the unseen oral tradition—our "Q" source—that fed into

[134] This theory is better explained and elaborated on in Alister McGrath, *Historical Theology: An Introduction to the History of Christian Thought* (Oxford: Blackwell Publishers Ltd., 1998), p. 39.

physics lore. Textbooks act like the extant biblical manuscripts that anchor certain passages to a specific place and time and, ultimately, to the origins of a myth like the plum pudding model.

CONCLUSION

At a time when American physicists were trying to stick to the science and avoid publicity, an otherwise shy Luis Alvarez started writing essays, speeches, textbooks, and an autobiography that attempted to establish him and his discipline as the premier American science. Some of it came naturally; the military, forever thankful for the atomic bomb, readily contributed to physicists' need for equipment. Some of it came slowly, as most physicists preferred to keep to themselves and some even regretted their role in creating the atomic bomb. Many simply did not have the facility with popular writing that emerged much later with the rise of such charismatic physicists as Richard Feynman and Freeman Dyson. Even these physicists took until the late 1970s or early 1980s to enter the public discourse. American physicists fully developed a public persona surprisingly late.

Arguably, this entire project is about recruitment. Any social group that lasts more than a generation must attract and groom the next generation. American physics was around long before the Manhattan Project and the rise of physics to preeminence among the sciences, but the character of the discipline certainly changed after that. Alvarez became an instructor at Berkeley in 1936 before he realized his future place in history. However, the goals for physics recruitment changed in the mid-twentieth century. Physicists had always tried to attract young students; textbooks are a key strategy in that endeavor. Textbooks adapted to the changing prominence of American physics by casting a wider net and trying to make the facts and equations into a compelling story with heroes and adventures. Alvarez's textbook draft is a particularly ambitious example of story telling in physics.

One would think that the transformation would have been complete soon after the bombing of Hiroshima, but we have seen that American physicists took another several decades to expand their story telling beyond the classroom to the public realm with popular autobiographies. There are several reasons for this delay, but today it is easy to forget that physics was not always the premier science. In fact, by the time physicists began writing autobiographies—the time when physicists realized or admitted that lay people might want to read their adventures—physics was already in relative decline. By 1979 when the Sloan Foundation's first Foundation Series of scientist autobiographies went to print, molecular biology had risen to prominence. By the early 1990s, the American physics community saw the cancellation of the Superconducting Super Collider project in Texas. Today, the Human Genome Project and other work in molecular biology garners much more attention than even Europe's Large Hadron Collider, an accelerator with less than half the power of the abandoned Texas accelerator.

As with the other chapters in this work, one of our goals here is to find new directions for research. The primary form of physics recruitment is the physics lecture. We have focused on textbooks because the written record they leave is much more easily studied. History is the written record. However, it would certainly be interesting to study the university physics class as a means of transmitting values and culture to the next generation of physicists. Historian David Kaiser's recent *How the Hippies Saved Physics* uses a promising collection of lecture notes from Cal Tech to follow trends in physics lectures. Perhaps further research will find value in the public speech, the letter to the editor, or even in science fiction writing and film. In 1963, Arthur C. Clarke wrote a novel, *Glide Path*, about Alvarez and his work on radar landing systems.[135] This work does not address that novel, but it is certainly a rich topic. In the conclusion, we will consider the significance of comic books in the perception of physics. A look at science in film would almost certainly be entertaining, if not insightful.

[135] Arthur C. Clarke, *Glide Path* (London: Sidgwick & Jackson, 1963).

Chapter 4: *Insider as Outsider*

Alvarez often crossed boundaries to work in fields outside high-energy physics. He had a very good nose for a hot field wherever that might take him. He was adroit at applying his skills as an experimental atomic physicist to other fields in ways that could not have occurred to people in that field. In Alvarez, we have a physicist who would eventually become better known to the public for his work on dinosaur extinction than for his Nobel work. This same work will also serve to introduce the next chapter on amateur theorizers who wrote to Alvarez.

He was very competitive and thought he could beat others at their own game; he generally assumed he would. If anything, he was surprised when he failed to improve on other fields. He recalled taking gymnastics in college: "When I watch gymnastics competitions today, I'm struck by the routines, most of which had not been invented when I was a gymnast. … Why didn't I invent any new gymnastic routines? I'm astonished that I could have spent ten hours a week for four years doing and watching gymnastics and never try to improve it."[1] This was one aspect of the dominance of atomic physics during the Cold War. Physicists could invade other fields like chemistry and molecular biology, applying the new quantum mechanics or particle accelerators to revolutionize those fields.

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[1] Luis Alvarez, *Alvarez: Adventures of a Physicist* (New York: Basic Books, 1987), p. 20.

In order to understand a social group, anthropologist Fredrik Barth argued that one must look at its relationship to other groups: "I urge us to focus the investigation on 'the ethnic boundary that defines the group, not the cultural stuff that it encloses.'"[2] He added "that these boundaries exist despite a flow of personnel across them."[3] Alvarez is just such a figure, a physicist who worked in forensics, pyramidology, and paleontology. By studying him, we can learn about the boundaries that Barth urged us to study. In that way, we can understand American atomic physicists as a social group. What did Alvarez think of scientists in other fields? What did they think of him and his intrusion into their field? In the next chapter, we will see what he thought of amateurs trying to enter his field. We will start with an example where Alvarez met with some resistance for physics work outside particle physics, but potentially on its fringes.

MAGNETIC MONOPOLES: A MINOR CONTROVERSY IN PHYSICS

Alvarez was involved in a curious search that, while certainly scientific, has remained on the borderlands of science. At the risk of oversimplifying a complex question, a magnetic monopole can be defined as a one-ended magnet. That is, if you cut a magnet into its north-south halves, the two halves each become whole magnets again with their own north and south poles. Physicists had long wondered if there could be a monopole which was only north or only south. However, it is a bit of a fringe interest. Alvarez entered the field and was chided by some colleagues over his experiment and

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[2] Fredrik Barth, ed., *Ethnic Groups and Boundaries: The Social Organization of Culture Difference* (Long Grove, Ill.: Waveland Press, 1998), p. 6.
[3] *ibid.*, p. 9.

publication in *Science*. Alvarez defended himself and, in doing so, helps us understand the line between mainstream science and the fringe.

Alvarez devised a nondestructive and extremely sensitive test for monopoles that could be done quickly and inexpensively on relatively large samples.[4] Searches on earth had all come up negative. After the Apollo missions began returning with moon rocks, Alvarez saw an opportunity. The moon's surface has been exposed to cosmic rays for billions of years, and it seemed possible that if monopoles existed at all, they might be embedded in moon rocks.[5] Alvarez's tests, however, did not show any evidence that the moon rocks contained any monopoles.

Soon after Alvarez and his colleagues published a short account of their lunar monopole experiment in *Science*, the work was criticized in *Nature*. A throwaway line in a four page opinion piece by the editors of *Nature* is quoted here in its entirety: "and is it really sensible to go looking for magnetic monopoles in samples of moondust recovered from the Moon this week?"[6] Alvarez responded with a letter to the editor explaining the importance of the work:

> Modern experimental physicists are generally agreed that the discovery of any one of the following would constitute a major breakthrough in our understanding of the physical universe: (1) free magnetic monopoles, (2) the heavy particles (quarks, etc.) whose existence has been postulated to explain the observed regularities in the 'spectroscopy of fundamental particles,' and (3) gravitons, or some observation of gravity waves.

[4] Luis W. Alvarez, Philippe H. Eberhard, Ronald R. Ross, and Robert D. Watt, "Search for Magnetic Monopoles in the Lunar Sample," *Science* 167, no. 3918 (30 Jan. 1970), p. 701.
[5] Philippe H. Eberhard, "Magnetic Monopoles" in Trower, *Discovering Alvarez*, p. 176.
[6] "Putting the Moon Where It Belongs: Defining Objectives" *Nature* 223, no. 5204 (26 July 1969), p. 336.

> Searches for physical phenomena of the kind just mentioned have a special fascination for experimental physicists in that if one multiplies the chance for success (which is admittedly very low) by the scientific importance (which is enormous), the product compares favorably with that of the more routine experiments on which one spends most of one's scientific life.[7]

In other words, Alvarez argued that the estimated value of looking for monopoles was substantial—while finding them is unlikely, the payoff is great. It is true that experimental evidence of magnetic monopoles would revolutionize the foundations of electro-magnetic theory; Maxwell's late nineteenth century theory establishing the electro-magnetic field might be upended. However, physics would be rocked by any number of unlikely finds.

It is difficult for a non-physicist to judge the merits of physics research, but over forty years of hindsight gives us an advantage in this debate. Alvarez was certainly right that the discovery of hypothesized heavy particles had a great impact on physics. Indeed, articles on the search for the Higgs boson and on the Large Hadron Collider litter the popular physics news today.[8] A search in the journal *Nature* for articles from January 1960 to December 1969—roughly the decade leading up to Alvarez's lunar monopole search—finds 287 articles containing "gravity waves," although it only returns seven articles containing "graviton."[9] This is, of course, only a very rough guide to the relevance of a particular topic. Those 287 articles include the 1965 article "Blood Flow

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[7] Alvarez, *Adventures*, p. 329. Originally in Alvarez, "Putting the Moon where it Belongs," (letter to the editors), *Nature* 223, no. 5210 (6 September 1969), p. 1082.

[8] For example, the New York Times wrote on the search for the Higgs boson at the new CERN accelerator eight times in calendar 2009.

[9] Note: "gravitational wave" returns 214 results, which may be a better guide, since waves in a pool of water could be considered "gravity waves"—waves driven by the crest's attraction to the earth. For more on gravitational waves, see Harry Collins, *Gravity's Shadow: The Search for Gravitational Waves* (Chicago: The University of Chicago Press, 2004).

In Turtles" for some reason this author could not determine. Further, an article shooting down a theory would still come up in a search for that topic. A search for "heavy particles (quarks, etc.)," in Alvarez's words, would be difficult to do, but a search for "magnetic monopole" returns only eighteen articles in the same time and journal. If publication frequency is any guide, monopoles do not appear to be as rich a field as gravity waves, despite Alvarez's hopes.

Alvarez's letter to *Nature* pointed out "the very recent discovery of gravity waves by Joseph Weber."[10] He continued, "I was pleased to be reminded of this sentence,"[11] referring to the big three searches as he sees them. Unfortunately, this discovery was itself questioned later.[12] Essentially, Weber's detection of gravity waves was not reproducible by any other physics teams. Richard Garwin, one of the physicists who helped discredit Weber's claim, reported in an interview that the issue is now settled, but "Weber is just such a character that he has not said, 'No, I never did see a gravity wave'" and that the NSF, "which funded that work, is not man enough to clean the record, which they should. So it's not a bright spot in the pages of modern physics."[13] Of course, Alvarez pointed out that a physicist should be able to admit his errors.[14] While Weber may never have admitted a mistake, there is no evidence that Alvarez made this mistake.

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[10] Alvarez, "Putting the Moon where it Belongs," p. 1082. Alvarez was referring to Joseph Weber, "Gravitational-Wave-Detector Events," *Physical Review Letters* 20, no. 23 (Jun. 1968), p. 1307.
[11] *ibid*.
[12] James Levine best summarized the critique and history of Weber's claim in Levine, "Early Gravity-Wave Detection Experiments, 1960-1975," *Physics in Perspective* 6, no. 1 (Mar. 2004), p. 42. According to Levine, he and Richard Garwin shot down Weber's discovery in the mid-1970s.
[13] Interview of Richard Garwin by Finn Aaserud on 23 October 1986 Session I, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.
[14] Alvarez, *Adventures*, p. 247.

Perhaps monopoles were not so "outsider" as they sound today. Alvarez pointed out that Paul Dirac, one of the founding fathers of quantum mechanics, provided a theoretical basis for the monopole partly in 1931 and more completely in 1948, tweaking Maxwell's equations to conform to the new quantum mechanics.[15] While Dirac did not use the term "magnetic monopole," instead choosing "a particle with a single magnetic pole," he did indeed describe a reason for experimentalists to hunt for them. Dirac noted in 1948 that "Since electric charges are known to be quantized and no reason for this has yet been proposed apart from the existence of magnetic poles, we have here a reason for taking magnetic poles seriously."[16] Another way of thinking of a magnetic monopole is the analogy with electric charge. Negative electric charge can be separated from a positive charge; rubbing a balloon on one's hair separates negative electrons from one's head. Maxwell's equations require that the net magnetic "charge" be zero—the hypothetical "north" magnetic charge cannot be separated from its "south." Why should this be? Perhaps the quantum mechanical revolution of the 1920s and 30s would provide an answer that involved previously undiscovered magnetic monopoles. However, the failure to find the monopole by the time of this writing makes that possibility seem unlikely.

In Alvarez's defense, while he may have been tinkering on the fringes of mainstream physics, his letter to the editors of *Nature* provide some solid reasons to

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[15] Paul Dirac, "Quantised Singularities in the Electromagnetic Field," *Proceedings of the Royal Society of London, Series A*. 133, no. 821 (1 Sep. 1931), p. 60 and Dirac, "The Theory of Magnetic Poles," *Physical Review* 74, no. 7 (1 Oct. 1948), p. 817.

[16] Dirac, "The Theory of Magnetic Poles," p. 817.

pursue this research. Besides not being alone in the search for monopoles, Alvarez points out that this search would do no harm:

> The search is non-destructive; it doesn't subtract a microgram from the available sample, and it didn't add a microgram to the weight of the lunar module, nor subtract a microsecond from the time the astronauts had to do science on the Moon.[17]

Alvarez's search was done with moon dust that had already been brought back for other purposes and would not damage them in any way. Further, Alvarez's technique was, by the standards of high-energy physics, very inexpensive.

One should not come away from the monopole search thinking Alvarez was obsessed with a fruitless search. In fact, Alvarez was later responsible for shooting down an alleged monopole discovery. Fellow Berkeley physicist Buford Price thought he had found a monopole in 1975 and told Alvarez, who congratulated him on his grand discovery.[18] Essentially, Price "caught his supposed monopole in a carefully designed stack of plastic sheets, nuclear emulsion, and supersensitive photographic film."[19] After much thought, Alvarez was able to produce an alternate explanation that required no undiscovered phenomena. It turns out that Price's experiment captured "a platinum nucleus fragmenting as it passed through his package of plastic sheets, first to osmium and then to tantalum."[20] For that explanation, the stack of sheets had to be thinner than Price had claimed, so Alvarez called Price's "collaborators to ask whether the package

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[17] Alvarez, "Putting the Moon where it Belongs," p. 1083.
[18] Alvarez, *Adventures*, pp. 238-241.
[19] *ibid*., p. 239.
[20] *ibid*.

wasn't really less than half the quoted thickness; the answer was yes."[21] Price was about to present at a conference at Stanford, so Alvarez tried to convince him that it was a platinum nucleus, not a monopole: "We talked for a solid hour, but I was unable to convince him."[22] Alvarez used his substantial status in the physics community to get a last-minute, blind paper submitted to the conference. "I said that since I hadn't learned anything new at a conference in thirty years I wanted the privilege Wilhelm Roentgen had enjoyed when he surprised his colleagues with news of X-Rays without preparing them in advance."[23] That is, a physicist of Alvarez's stature never heard something new at a conference without hearing about it from the rumor mill beforehand and he thought he deserved the privilege of the great discoverer of X-rays, Wilhelm Roentgen. Alvarez gave his talk[24] disproving Price's claim as diplomatically as he could, thanking Price for "his complete openness and obvious desire to have all the facts of the case made known."[25] Price cabled the moderator that he conceded Alvarez's critique and for this bit of work, Alvarez got a compliment from the physicist Richard Feynman: "That was a great detective job!"[26] Alvarez was clearly interested in finding the truth and not merely engaging in a blind hunt for monopoles. Further, this story shows in part the respect he demanded and got for his years in the physics community.

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21 *ibid*., pp. 239-240.
22 *ibid*., p. 240.
23 *ibid*.
24 Alvarez, "Analysis of a Reported Magnetic Monopole," Invited talk presented at the Stanford International Conference on Leptons and Photons, Stanford, CA. 16 September 1975.
25 Alvarez, *Adventures*, p. 241.
26 *ibid*.

Perhaps we could learn from Michael Shermer's analysis of the demarcation problem in his approachable *The Borderlands of Science: Where Sense Meets Nonsense*.[27] Shermer, who edits *Skeptic Magazine*, argues that the demarcation between science and non-science is not sharply defined. Instead, he constructed a scale from .1 to .9, where .1 is the lowest possible score and .9 is the highest. Examples of what he calls "Normal science" include heliocentrism, evolution, and quantum mechanics. Clear examples of "Nonscience" include creationism, holocaust revisionism, and UFOs. Where Shermer's analysis becomes useful for our purposes is his category of "Borderlands science" that includes superstring theory, inflationary cosmology, and SETI, the Search for Extraterrestrial Intelligence. Superstring theory is certainly scientific in that it adheres to the practices of science and is practiced by physics insiders, but it remains unproven. As for SETI, Carl Sagan and Philip Morrison were among the many respectable scientists who used radio telescopes and massive, distributed computing to search for radio signals from intelligent extraterrestrial life. This work is not mainstream science in that it is highly speculative, but the principles are sound. They are not UFO "nuts." In this sense, Alvarez can be a physics insider working on borderlands science. Ultimately, a borderlands science must become normal science or be abandoned, but given the potential for discovery afforded by the Apollo missions, Alvarez was playing the game by the rules.

[27] Michael Shermer, *The Borderlands of Science: Where Sense Meets Nonsense* (Oxford: Oxford University Press, 2001), "Blurry Lines and Fuzzy Sets," especially p. 23.

THE KENNEDY ASSASSINATION AND THE "BUFFS"

In the years after President John Kennedy was assassinated in 1963, a great number of conspiracy theories developed in opposition to the Warren Commission and its single gunman theory—the official line that Lee Harvey Oswald, acting alone, shot and killed President Kennedy from the Texas Book Depository.[28] Kennedy had been a hero to Alvarez, who was, like most Americans, shocked and hurt by the assassination.[29] In fact, Alvarez had met Kennedy twice. The first time, the President asked him if he was related to Dr. Walter Alvarez of the Mayo Clinic; Luis was of course Walter's son.[30] Years earlier, Luis's father, Walter, had invited President Kennedy to lunch after examining him at the Mayo Clinic. Alvarez met Kennedy again in 1962 when the president awarded the Collier Trophy to the Mercury astronauts. Alvarez would have met Kennedy a third time when he received the National Medal of Science, but it would be "an appointment abruptly canceled in Dallas. Lyndon Johnson did the honors later."[31] Alvarez attacked the problem of explaining the counterintuitive evidence surrounding the assassination like a physicist, eventually publishing an article in the *American Journal of Physics* on the merit of demonstrating a sound scientific method in everyday problem

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[28] By December 1966, *Esquire* cited 35 distinct theories critical of the Warren Commission report and that was before the influential *Six Seconds in Dallas* by Josiah Thompson. Luis W. Alvarez, "A Physicist Examines the Kennedy Assassination Film," *American Journal of Physics* 44, no. 9 (Sep. 1976), p. 813. Reprinted in Trower, ed., *Discovering Alvarez*. The pagination here will reflect that in Trower.

[29] Alvarez, *Adventures*, 244.

[30] *ibid*., 222.

[31] *ibid*.

solving.[32] All in all, Alvarez's work is cited as authoritative in Gerald Posner's 1993 book on the Kennedy assassination, *Case Closed: Lee Harvey Oswald and the Assassination of JFK*.[33] The Kennedy assassination work became a significant part of Alvarez's autobiography—warranting an entire chapter, "Scientific Detective Work"—and it provides an opportunity for us to understand his relationship with the "buffs."

Alvarez became aware of the conspiracy theories through his student Paul Hoch. Were it not for Hoch, Alvarez said he would never have been aware of the thorough literature questioning the government's story. Hoch earned his Ph.D. in Alvarez's lab and was Alvarez's "main contact with the buffs" and "devoted all his spare time for many years to buff activities."[34] Later, Hoch would co-edit *The Assassinations: Dallas and Beyond: A Guide to Cover-Ups and Investigations* with Peter Dale Scott and Russell Stetler and became something of an expert on Kennedy assassination theories.[35] Hoch published a newsletter on conspiracy theories, *Echoes of Conspiracy*.[36] Apparently, "buffs" like Hoch did not object to the term. He was most well known to assassination researchers for arguing that the more extreme theories took credibility away from legitimate research towards a better understanding of the assassination. Hoch introduced

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[32] The editor of the *American Journal of Physics* included an editor's note explaining the article's "unique pedagogic usefulness" while noting that "We do not feel that this journal is an appropriate forum for a discussion of alternative theories of the assassination." Alvarez, "A Physicist Examines," p. 210.

[33] Gerald Posner, *Case Closed: Lee Harvey Oswald and the Assassination of JFK* (New York: Random House).

[34] Alvarez, *Adventures*, p. 243.

[35] Peter Dale Scott, Paul Hoch, and Russell Stetler, *The Assassinations: Dallas and Beyond: A Guide to Cover-Ups and Investigations* (New York: Random House, 1976). Hoch gave a 1993 talk at the Second Annual Midwest Symposium on Assassination Politics in Chicago where the editor said he was not a "high-profile conspiracy author, … but, among the small fraternity of assassination researchers, he is a highly respected figure." Paul Hoch, "Winnowing the Wheat and the Chaff," http://mcadams.posc.me.edu/hoch.htm. Retreived 7 July 2010.

[36] Alvarez, *Adventures*, p. 243.

Alvarez to the problem of President Kennedy's head snap in the Zapruder film. Alvarez initially dismissed the apparent contradiction of the president's head moving back if the shot came from behind, thinking it was Kennedy going limp. Hoch lent Alvarez *Six Seconds in Dallas* by Josiah Thompson and Alvarez realized that "the assassination buffs were right; there had to be … some real force"[37] driving the president's head back. "I then had to find an explanation for that force."[38] Although Hoch was also a Ph.D. physicist, the brilliant Alvarez was able to bring a level of sophistication to the analysis that the buffs could not match.

Alvarez's involvement in the Kennedy assassination investigation came in two parts and had four major results. Initially, Hoch and Alvarez ran an informal investigation in 1969 that explained the head snap and determined the timing of the shots, in the process finding a way to measure the speed at which the Zapruder film was recorded. Later, in 1976, Alvarez was called to Washington to participate in a commission to investigate the audio recordings of the assassin's shots. Alvarez published the results of all of this work in an article for the *American Journal of Physics*, "A Physicist Examines the Kennedy Assassination Film" and summarized the work in his autobiography. While that section of the autobiography was only nine pages, it comprises the majority of chapter fourteen, suggesting that it was important to Alvarez.

The first bit of detective work was explaining the counterintuitive head snap—logic would suggest that a shot from Oswald's position behind the president would push

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[37] *ibid*.
[38] *ibid*.

his head forward, but the Zapruder film clearly shows Kennedy's head moving backward after being struck. Since this is not a forum for Kennedy assassination theories, we will describe Alvarez's work only briefly, but the work does provide an insight into Alvarez's personality and his intellectual style. As he said when he first decided to figure out the head snap, "Since I knew more physics than the buffs, it didn't take me long."[39] Alvarez's explanation was that the jet of brain matter that exited the front of the president's head imparted more momentum to his head than the bullet had. Like a true physicist, but also like a man who had lost a hero, Alvarez created an abstracted model to test his theory: "I found it painful to consider in detail what happened to my hero John Kennedy, and thought of the problem instead as an abstract experiment involving melons reinforced with tape."[40] Alvarez and a Radiation Laboratory associate, Sharon "Buck" Buckingham, tested the theory at a shooting range. One gun enthusiast present said, "I've been around guns all my life … and you must be out of your mind to believe something you hit with a bullet will come back at you."[41] Yet the melons did snap backwards toward the rifle. Further forensic tests by physician John Lattimer—"the first private doctor allowed to view the autopsy X rays and photographs"—confirmed Alvarez's "jet effect."[42]

The Warren Commission decided that there were three shots despite reports ranging from two to six, but they did not establish with certainty the order of the shots,

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[39] *ibid*.
[40] *ibid*., p. 244.
[41] *ibid*., pp. 244-5.
[42] Posner, *Case Closed*, pp. 306, 316.

despite studying the Zapruder film.[43] Alvarez knew optics; he had invented the first inertially stabilized lens of the type common in cameras today.[44] A close look at the assassination film showed that amateur cameraman Abraham Zapruder was an excellent "tracker"—that is, he had a very steady hand—with the exception of three moments, each about five frames after a gunshot. Alvarez was able to study reflections that appeared as points when Zapruder tracked well, but were rendered as streaks when the camera shook. This way, Alvarez was able to show that there were exactly three shots: the first missed, the second passed through Kennedy and hit Governor Connally of Texas, and the last one hit Kennedy in the head.

A related problem involved establishing the camera's speed.[45] The model of Bell-Howell camera that Zapruder used had two settings: an eighteen frames per second "normal" speed and a forty-eight frames per second "slow motion" setting. If Zapruder had been shooting in the slow motion setting, the sequence of shots would be all but impossible for a single shooter. Here, Alvarez applied physics to physiology. With the Zapruder film, physiology came to his rescue as Alvarez found a clapping man in the crowd. Alvarez counted the number of frames between claps—between 3½ and four claps per second—and calculated the force needed to square that with both of the possible film rates. While the average observer would simply try to clap fast enough for a forty-eight frame-per-second film speed, Alvarez showed that the force required increases with

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[43] Alvarez, "A Physicist Examines," pp. 210-216.
[44] Alvarez, "Gyroscopically Controlled Accidental Motion Compensator for Optical Instruments" Patent no. 3,378,326; April 16, 1968 and two other patents improving on this.
[45] Alvarez, "A Physicist Examines," pp. 219-220.

the cube of the frame rate, which quickly became impossible for more than about twenty frames per second. He backed that up with a test using a metronome, but not before making the problem perhaps more mathematical than it needed to be, complete with a graph of claps versus time.[46]

Alvarez continued the analysis of timing in the Zapruder film by studying the velocity of the presidential limousine where the FBI had failed.[47] An FBI photoanalyst testified that there was no way to determine the speed of the limousine because there was no reference point, "There is just a grassy plot."[48] Alvarez showed that a person standing with his weight on one leg provided a perfect, stationary reference point: his foot. By studying the camera motion from frames 160 to 340, Alvarez showed that the limousine slowed at around frame 300 from roughly twelve miles per hour to about eight. The evidence for such a deceleration, while counter to the driver's testimony, seems conclusive and would have taken place between the first and second shot, allowing Oswald an easier final shot at the president. Of course, Alvarez did so with a thoroughly mathematical approach, graphs and all.

Finally, while not specifically Alvarez's work, a committee he formed poked holes in the buffs' audio evidence. A sound recording was discovered that apparently recorded the shots and a number of echoes. National Academy of Sciences president Phil Handler called Alvarez to investigate the tapes for Congress in 1976. Alvarez chose not to head the committee, arguing that "the buffs would automatically have rejected any

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[46] Fig. 5 in Alvarez, "A Physicist Examines," p. 220.
[47] Alvarez, "A Physicist Examines," pp. 220-223.
[48] *ibid.*, p. 220.

report published under my name."[49] Apparently, Alvarez was anathema to the buffs. Alvarez suggested physicist Norman Ramsey to head the committee. Ramsey had worked at the Manhattan Project, had distinguished himself with his work on molecular beams at Harvard and would later win a Nobel Prize in physics in 1989. Alvarez hoped to raise the scientific standards of the committee. He was unimpressed with the expert witnesses: "I found the testimony of the acoustics experts in the House subcommittee report amateurish."[50] At one point, an acoustics expert suggested that the sound wave would be concave, while "Even a high school physics student would know that almost all such wave fronts must be convex, but the expert refused to agree that anything was wrong with his drawing."[51] Alvarez concluded that a professional expert witness could never admit being wrong even once, since that transcript would be read at every future trial where he testified. "It was a new experience for me to watch a Ph.D. physicist stonewall in a technical argument."[52] Alvarez was applying the standards of academia, where a physicist often makes mistakes—educated guesses at a theory—and is ready to admit being wrong. It would seem that an advanced degree in physics and a court's designation of "expert" would not be the arbiter of authority, at least not when that expert is being grilled by a Nobel laureate. Ultimately, the National Academy of Sciences

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49 Alvarez, *Adventures*, p. 247.
50 *ibid*.
51 *ibid*.
52 *ibid*.

committee determined that the motorcycle was not in Dealey Plaza when its microphone was recording.[53]

Involvement in the Kennedy assassination investigations proved frustrating for Alvarez because the scientist is supposed to be able to admit when he or she is wrong. The Kennedy assassination "buffs" did not live up to Alvarez's standards of science; theirs was a pseudoscience. Yet the episode is interesting in that Alvarez was on their turf. The assassination of a widely beloved president should be the domain of every citizen, but the buffs dominated the debate and stubbornly resisted Alvarez's logic. But by joining the debate, was Alvarez a conspiracy buff? He was certainly no conspiracy theorist in that he did not believe the alternative theories and conspiracies. Yet his enthusiasm for the subject made him a bit of a buff. Biographer Richard Rhodes told me that "Luis always kept one large foot in the world of self and identity and ambition and solving puzzles and got a real kick out of figuring out the Kennedy assassination."[54] He was certainly an enthusiast in that he devoted a good deal of time to the issue and he was proud of his work, naming a chapter of his autobiography "Scientific Detective Work" after the assassination work. Oddly, Alvarez received very few letters from Kennedy assassination conspiracy theorists. Perhaps they knew better than to challenge him and instead avoided his work to prevent being embarrassed by his systematic logic.

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[53] *ibid*., p. 250.
[54] Richard Rhodes interview by author, 20 May 2009, p. 40.

ALVAREZ AND ALVAREZ ON DINOSAUR EXTINCTION

For all his work on hydrogen bubble chambers and the Nobel Prize it earned him, the Manhattan project, radar, his aviation work, all his patents, and other achievements this work will attempt to document, Luis Alvarez will probably be best remembered[55] for a 1980 paper in which he; his son, Walter; and Berkeley chemists Frank Asaro and Helen Michel announced their discovery of the extinction of dinosaurs by a great asteroid impact.[56] Generally accepted now, the idea that the dust kicked up by a meteorite could cause the great extinctions at the end of the Cretaceous period was an outsider theory to paleontologists in those first few years after its introduction. It was an extra-terrestrial explanation for a longstanding mystery in an established earth science—arguably a sure sign of a "nut." Further, Walter thought they would "get in trouble with many paleontologists, who did not think a geologist, a physicist, and two nuclear chemists should be trespassing in someone else's area of science."[57] They were outsiders to the field of paleontology who would turn the field on its head without any previous history of publishing work in that field. Finally, that the geologist on their team was Luis's oldest son provided another twist that both humanizes the participants in this story and adds to the storybook aesthetic of this tale of cataclysm, astronomy, and dinosaurs.

Luis's son from his first marriage, Walter Alvarez, had gone to Princeton to get a Ph.D. in geology. Luis had not thought much about geology before that. However, in

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55 He acknowledged as much to biographer Richard Rhodes. Rhodes interview by author, 20 May 2009, p. 9.

56 Luis W. Alvarez, Walter Alvarez, Frank Asaro, and Helen V. Michel, "Extraterrestrial Cause for the Cretaceous-Tertiary Extinction: Experimental Results and Theoretical Interpretation," *Science* 208, no. 4448 (6 Jun. 1980), p. 1095. Reprinted in Peter Trower, ed., *Discovering Alvarez*, p. 243.

57 Walter Alvarez, *T. rex and the Crater of Doom* (Princeton: Princeton University Press, 1997), p. 75.

1977, Walter returned to academia after a stint in petroleum engineering and got an assistant professorship at Berkeley. Shortly after Walt returned to Berkeley, he brought his father "a complicated rock the size of a cigarette package that was preserved in transparent plastic"[58] that he had excavated in Gubbio, Italy. There were two layers of limestone separated by a thin layer of clay. Below the layer of clay, there were fossils from the Cretaceous period embedded in white limestone; above it was red limestone with no fossils. This clay layer, Walt told Louie, was present everywhere on earth and separated the Cretaceous era fossils from the later Tertiary period.[59] Luis said "that was one of the most fascinating revelations I'd ever heard."[60] That nobody knew why this thin clay layer separated the two eras or how it was formed would obviously make for a great detective case that the two Alvarezes would famously solve.

Luis said, "Walt's decision [to move to Berkeley] rejuvenated my scientific career."[61] This new challenge piqued the senior Alvarez's competitive nature. He said that he "had not found Walt's field of science exciting before; now I did."[62] Walter noted in his popularized account of his impact work that "Dad did not originally think that geology was an interesting science,"[63] noting that it was his mother, Luis's first wife, who nurtured his interest in geology. Walt added that through college, graduate school,

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[58] Alvarez, *Adventures*, p. 252. I do not have the exact date that Walt showed Luis the Gubbio rock sample, but Walt characterized it as "Shortly after I returned to Berkeley," which was in the fall of 1977. Walter Alvarez, *T.Rex and the Crater of Doom*, p. 63.

[59] The lower tertiary period is now called the Paleogene era and the boundary is now called the Cretaceous-Paleogene or K-Pg. Here we will stick to the K-T terminology that was in use when Alvarez and Alvarez set out on this research.

[60] Alvarez, *Adventures*, p. 252.

[61] *ibid*.

[62] *ibid*.

[63] Walter Alvarez, *T. rex and the Crater of Doom*, p. 60.

and his work in petroleum engineering, "I rarely saw my father, and did not know much about him as a scientist."[64] Richard Rhodes remembered a stronger version of that story: "when Luis understood the anomaly here, he said, 'I looked at Walter and said, "Huh, maybe geology is actually a science after all!"'"[65] This implies a very different approach to the demarcation problem indeed, but Alvarez was almost certainly exaggerating for effect. Perhaps his son's geology was not *as much of* a science as high-energy physics in his mind. Once again, he would be able to use his considerable skills and resources to solve the problem of dinosaur extinction.

Initially, Alvarez and Alvarez wanted to figure out how long it took for the clay layer to form. That would help determine if the layer formed from a slow, sedimentary process or a fast, cataclysmic event. Luis decided that they might be able to solve that question by looking at how much interstellar dust was in the layer. To understand his idea, one needs to understand two things. First, a number of iron-loving or siderophilic elements are depleted in the Earth's crust, such as cobalt, nickel, and the rarer iridium. That is, any element that is chemically attracted to iron would have been absorbed into the iron-heavy core of the earth when the earth was forming. Therefore, these elements are relatively rare on the earth's crust and are found mainly in the core or in asteroids, which were not exposed to the earth's molten core. Second, space dust and debris are constantly falling to the earth at a rate of about ten million kilograms per year.[66] This

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64 *ibid*.
65 Rhodes interview, 20 May 2009, p. 32.
66 Frank Asaro, "The Cretaceous-Tertiary Iridium Anomaly and the Asteroid Impact Theory," in Trower, *Discovering Alvarez*, p. 240.

material has a normal distribution of elements, and so has a higher concentration of siderophilic elements than does the earth's crust. Luis decided that they could measure the concentration of one of these elements to decide how long the clay layer was exposed to the constant rain of space dust. After some thought, they decided on iridium because they thought it would be the easiest to detect.

They asked Berkeley chemist Frank Asaro to help with the analysis in early October 1977. Although Asaro was not initially enthusiastic about looking for iridium due to earlier failures, he was interested in working with Luis Alvarez after reading about his pyramid work.[67] The trick would be to irradiate the material with neutrons. Iridium has a very large neutron cross-section, which means exactly what it sounds like: iridium nuclei are big targets for passing neutrons and absorb them easily. The target material would then be radioactive in a predictable way. If the radiation coming off the material had the signature of radioactive iridium, then they had found the space debris they were seeking. They expected a concentration of about 0.08 parts per billion (ppb); the first sample they tried from Gubbio, Italy, had about nine ppb! They were hoping to find iridium, but were puzzled by this enormous concentration—over one hundred times the expected amount. Something besides the usual space dust had found its way into the clay K-T boundary.

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[67] *ibid.*


Figure 4.1: Luis and Walter Alvarez in Gubbio, Italy examining the K-T boundary, 1981.

Alvarez, Alvarez, and Asaro decided they would need to test many more samples to see if this was an anomaly restricted to Italy. Asaro asked his "nuclear chemist associate for 25 years"[68]—Helen Michel—to help. By the time the team's paper was published in 1980, they had only managed to test samples from Italy and Denmark, but other teams had confirmed the results in Canada and New Zealand. We now know that the layer is found all over the world, so we shall return to the team's work to explain why.

Luis Alvarez set out to come up with explanations. He later said that "Frank Asaro has recalled that I invented a new scheme every week for six weeks and shot them down one by one."[69] Some of these explanations included the earth passing through an interstellar dust cloud, a nearby super nova, and a large comet grazing the earth's atmosphere. Meanwhile, Asaro and Michel analyzed as many samples as they could. Asaro recalled that "For 36 hours Helen and I continuously ran chemical separations, while Luie and Walt fed us pizza, snacks, and chili."[70] For Luis and Walter, this project must have been a unique and welcome opportunity to bond. Luis's first marriage to Walt's mother, Gerry, had ended in a messy divorce. Luis did not talk about that much, but noted in his autobiography that "During the war I worked under strict secrecy rules for five years; Gerry and I forgot how to share our lives. I was also too frequently away and couldn't afford long-distance calls, which were expensive then."[71] The iridium work

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[68] *ibid*.
[69] Alvarez, *Adventures*, p. 256.
[70] Asaro, "The Cretaceous-Tertiary Iridium Anomaly," p. 241.
[71] Alaverz, *Adventures*, p. 281.

would be different for the Alvarez family, as Asaro would later recall: "Walt's wife Milly even dropped by with strawberry ice cream and chocolate chip cookies during one long night."[72] However, Luis was very competitive, even with his son, according to Rhodes.[73] One can only guess at how close the Alvarezes became during this work, but their relationship was chilly afterwards.

Walter Alvarez knew the potential significance of their work and proceeded carefully. One early idea suggested that plutonium they found in the clay layer meant that a supernova may have killed the dinosaurs. Walter asked for advice from Earl Hyde, a nuclear chemist and Deputy Director of the Lawrence Berkeley Laboratory. He said, "Do it all over again. … Repeat every single step from the very beginning, on a fresh sample, to be absolutely sure there really is plutonium-244 in that clay."[74] There was none in the second run. "We went home in the early morning dejected, but Earl Hyde saved us from the humiliation of having to retract a spectacular mistake."[75] After crossing a supernova off a list of possible causes, Luis Alvarez continued his brainstorming.

Luis finally hit on a plausible theory after studying the work of the Victorian physicist Sir George Gabriel Stokes.[76] In 1888, Stokes published a study of the large Krakatoa volcanic explosion of 1883. Stokes studied the rate at which a dust particle falls through air and guessed at the particulate size of the volcanic dust by looking at the

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72 Asaro, "The Cretaceous-Tertiary Iridium Anomaly," p. 241.
73 Rhodes interview. 20 May 2009.
74 Walter Alvarez, *T. rex*, p. 74.
75 *ibid*.
76 Alvarez, *Adventures*, p. 256.

sun through the haze. He was able to show that the dust should settle in two years, matching observations. Luis took that approach and imagined a large asteroid hitting the earth and sending a large plume of dust into the sky, blocking the sun and causing mass extinctions. None of the other theories had panned out, but this one was promising. However, not much was then known about the effects of such a dust cloud. The Alvarez theory required a lot of study beyond Stokes' work and an examination of a hypothetical cataclysm many times more powerful.

If the idea of cataclysmic explosions threatening life on earth sounds familiar, it certainly did to Alvarez. One is reminded that the asteroid hypothesis predated our theories of nuclear winter.[77] The idea of a nuclear winter got a boost from the work done to simulate the effect of a massive asteroid impact. The same computer models could help model the dust thrown into the atmosphere be it from an asteroid or a nuclear blast. Alvarez, the man who invented the detonators that made the implosion atomic bomb possible; who was one of the few people who witnessed the nuclear blasts at Trinity, Hiroshima, and Nagasaki; and who pushed for the construction of the hydrogen bomb, also created the impact theory that bolstered the theory of a nuclear winter. As if the devastation of thousands of atomic bombs were not enough, a complete nuclear exchange between the U.S. and U.S.S.R. could drive man extinct. In 1985, Senate Committee on Armed Services chair Barry Goldwater—a nuclear hawk if ever there was one—opened hearings on the possibility of a nuclear winter "by stating that he was 'a firm believer that

[77] See Lawrence Badash, *A Nuclear Winter's Tale: Science and Politics in the 1980s* (Cambridge, Mass.: The MIT Press, 2009).

there could be such a thing as a nuclear winter,' a conclusion based on recent discussions with the physicist Luis Alvarez about the asteroid that killed the dinosaurs."[78] Alvarez concluded his 1987 autobiography thusly:

> Soon after my colleagues and I published our impact hypothesis, a group of atmospheric experts at the NASA Ames Laboratory examined it in detail. They confirmed our general conclusions but thought the dust cloud would fall out more quickly than we had predicted. A study that grew out of that work is the now-famous "nuclear winter" paper that proposed that smoke from fires set by exploding nuclear weapons would similarly block out sunlight worldwide with consequences similarly dire. …
>
> I was already convinced that there are enough good reasons not to start a nuclear war. But the fact that neither of the two superpowers' nuclear-weapons establishments had thought about the possibility of a nuclear winter has sobered everyone concerned with fighting a nuclear war. What else, they wonder, have they forgotten to think about? The most encouraging feature of the nuclear-winter scenario is that no one has been able to disprove it. It has had a very salutary effect on the thinking of military planners on both sides of the world. There are some indications that it is weakening the Soviet military's long-held belief that nuclear war is survivable. If so, then it may turn out that my rather peculiar way of thinking about new problems will have had an important effect on what I continue to believe is the world's number one problem, the avoidance of nuclear war.[79]

Human-caused weather change is still an important topic after the end of the Cold War. Alvarez, who died in 1988, could not have foreseen that a decade into the next century, we would be vigorously debating a related idea—that carbon dioxide emissions may warm the planet.[80]

In 1979, when the asteroid team studied the effects of a large asteroid impact, they did not know if it would make the planet hotter or colder. Nobody had studied the

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[78] *ibid*, p. 171.

[79] Alvarez, *Adventures*, 282-3.

[80] The debate over man-made global warming from carbon dioxide emissions goes back to the early nineteenth century and Joseph Fourier, but certainly not with the urgency of today's debate. See Spencer Weart, *The Discovery of Global Warming* (Cambridge, Mass.: Harvard University Press, 2008).

hypothetical situation. Perhaps the cloud would block the sun and the planet would get cold, killing the dinosaurs. Perhaps the cloud would capture the sun's rays and make the planet too hot for dinosaurs. What was clear is that the strike would have been massive. At the height of the Cold War, the U.S. and U.S.S.R. had about fifty thousand nuclear weapons ready to fire, totaling the equivalent of roughly ten thousand megatons of TNT. The asteroid team decided that the impact that impregnated the K-T boundary clay with that much iridium must have been ten kilometers in diameter and had ten thousand times more destructive power than all those atomic weapons—100,000,000 megatons.[81] The press often focuses on the extinction of dinosaurs because they are popular and well known, but the K-T extinction event was massive and went far beyond just dinosaurs. Several species go extinct every year. Related species are lumped into a genus. Related genera comprise a family, which in turn form an order. Rodents are an order. Moths and butterflies together form an order. We belong to the order primate. During the K-T extinction, several orders went extinct.[82] Paleontologist Dale Russell of the Canadian National Museum of Natural Sciences in Ottawa "estimated that almost half the genera of animals, plants, and single-celled organisms had died out at the end of the Cretaceous.[83] Carl Orth, from the Los Alamos National Laboratory, showed that the pollen concentration above the iridium layer was 1/300 the concentration below.[84] The impact was big and it would take a lot of work to understand its effect.

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[81] Alvarez, *Adventures*, p. 259.
[82] *ibid*., p. 260.
[83] Walter Alvarez, *T. rex*, p. 75.
[84] *ibid*., pp. 259-60.

Luis Alvarez's autobiography gives a thorough account of how his group went about convincing the scientific world. Here we will settle for acknowledging the significance of the theory and note a couple of interesting anecdotes. The team published "Extraterrestrial Cause for the Cretaceous-Tertiary Extinction: Experimental results and theoretical interpretation" in the 6 June 1980 issue of *Science*. By the time of Luis Alvarez's death in 1988, the theory was well established. He noted that it was a "fine example of team science. We needed Walt's geological expertise, Frank's and Helen's nuclear and chemical competence, and my background in physics and astronomy."[85] It certainly helped that before the theory was proposed, paleontologists had not thought to use nuclear chemistry to their advantage. Who would have thought to bombard clay with neutrons years before such techniques were known outside of high-energy physics and nuclear chemistry? Perhaps the theory is a fine example of interdisciplinary work, but it is also a testament to Luis's problem-solving skills and a bit of his insider's powers. A young and unproven physicist-geologist team would have had a hard time convincing a chemist to join their team. Asaro noted that he was skeptical about joining the team. His earlier searches for iridium in pottery shards had turned up negative. However, Asaro did join: "I thought it would be interesting to work with Luie, as I had been intrigued by [Luis's] cosmic-ray search for undiscovered chambers in the Egyptian pyramids."[86] Later, Luis's prestige in the physics community would once again be useful:

> When we sent the paper to *Science*, Phil Abelson, the editor, returned it. It was too long, he said, and *Science* had published several papers in recent years

[85] Alvarez, *Adventures*, p. 257.
[86] Asaro, "The Cretaceous-Tertiary Iridium Anomaly," p. 240.

> purporting to explain the K/T extinction. "At least n-1 of them must be wrong," Phil remarked. He finally agreed to publish a shortened version of our paper, a courageous decision that earned our gratitude.[87]

In his autobiography, Alvarez did not need to fill in the blanks. In an earlier chapter, Alvarez told a story about the day early in 1939 when he heard that German scientists had split the uranium nucleus, while he was in the middle of getting a haircut in Berkeley:

> I stopped the barber in mid-snip and ran all the way to the Radiation Laboratory to spread the word. The first person I saw was my graduate student Phil Abelson. I knew the news would shock him. "I have something terribly important to tell you," I said. "I think you should lie down on the table." Phil sensed my seriousness and complied. I told him what I had read. He was stunned; he realized immediately, as I had before, that he was within days of making the same discovery himself.[88]

There is no evidence that, when Alvarez sent his paper on the asteroid theory to *Science* in 1980, Abelson simply complied with his old advisor's wishes purely out of loyalty, but it could not hurt Alvarez to have friends in high places. Most physicists trying to publish outside their field in a journal with the prestige of *Science* would not have had the editor's ear.

Of course, there is always the temptation to take a theory beyond its predictive powers. After developing confidence in the power of the asteroid impact theory, it seems logical to try to connect other impacts with mass extinctions. The K-T extinction was not the only one known to paleontologists; the Chicxulub impact near the Yucatan peninsula now believed to have killed the dinosaurs was not the only large asteroid impact. Two Chicago paleontologists showed that mass extinctions happen roughly every twenty-six

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[87] Alvarez, *Adventures*, p. 257.
[88] *ibid.*, p. 72.

million years. Luis Alvarez's physics graduate student, Richard Muller, realized that there was a rough periodicity to large asteroid impacts of approximately 28.5 million years. They hoped that they could find some sort of correlation—maybe there was some astronomical cause to many of the earth's mass extinctions. Muller has predicted a dark companion star he named Nemesis that would orbit the sun every 28.5 million years, causing a rain of debris every time it passed near an asteroid belt.[89] However, Luis pointed out that the difference between twenty-six and 28.5 was large—they would go out of phase every ten cycles. "Everybody will laugh at you,"[90] Luis warned Walter and Muller. Whether he meant literal or figurative laughter, even the Nobel Laureate Alvarez feared the public humiliation that amateur theorists associate with the heroic outsider. Alvarez disagreed with the those amateurs on being laughed at. In his many years of doing high-quality science at the highest levels, Luis Alvarez had a keen sense of what is possible in science, of what is good science. The Nemesis star has still not been found and the theory remains unresolved.

To Luis Alvarez's annoyance, his team spent much of its time dealing with criticisms from paleontologists who were disturbed by the very idea of a sudden extinction. He wrote to Joel Snow, the director of the Science and Technology Affairs Staff at the Department of Energy, "The main problem with 'the opposition' is that they

[89] Richard Muller, *Nemesis The Death Star: The Story of a Scientific Revolution* (New York: Weidenfeld & Nicolson, 1988).
[90] *ibid*., p. 266.

cause us to waste a lot of time answering their arguments."[91] However, to understand the criticisms, it is helpful to look at the background to the impact theory: gradualism vs. catastrophism. The story is much discussed and need not be discussed here beyond the basics.[92] Essentially, Darwin was influenced by the geologist Charles Lyell, who, in turn, was influenced by James Hutton, who believed that geological formations were built by slow processes like erosion and never (or very rarely) by catastrophic events that were reminiscent of the biblical tale of Noah and the Ark. Evolutionary biologists had, as part of their founding paradigm, a belief that evolution had to happen gradually[93] and their beliefs were given a boost by the discovery of radioactivity that implied the roughly five billion year age of the earth. By the early twentieth century, catastrophism smacked of theology, while gradualism had the approval of modern physics. In another boost to the entrenched uniformitarian paradigm, plate tectonics had revolutionized geology in the 1960s; what could be more uniformitarian than oceans spreading "at a rate of a few centimeters per year—about the rate at which your fingernails grow"?[94] Uniformitarianism seemed to be unquestionable. Zoologist Stephen Jay Gould

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[91] Alvarez to Joel A. Snow, Director, Science and Technology Affairs Staff, DoE, 22 Aug. 1983, LWAP box 40, folder "S 1987-87 [2 of 3]."

[92] See, for example, Peter Bowler, *Evolution: The History of an Idea* (Berkeley: University of California Press, 1989) and Stephen Jay Gould, *Punctuated Equilibrium* (Cambridge, Mass.: The Belknap Press of the Harvard University Press, 2007).

[93] Gould concedes that Darwin believed in gradual evolution, but that his friend Thomas Henry Huxley tried to convince him to leave the question of the pace of evolution open. That is, that evolution by natural selection did not *have to* be gradualist. See Gould, "The Episodic Nature of Evolutionary Change" in *The Panda's Thumb*. Gould further notes that in the Soviet Union, dialectical laws encouraged biologists to argue for sudden transformations like Gould's punctuated equilibrium.

[94] Walter Alvarez, *T. rex*, p. 56.

encountered this difficulty when he promoted the idea of "punctuated equilibrium."[95] That theory posits that evolution can sometimes happen quickly in cases where a small subset of the population is separated from the main group, such as after an earthquake or when birds are stranded on an island. Gould said the theory, which was bolstered by the Alvarez impact hypothesis, "provoked a major brouhaha, still continuing, but now in much more productive directions."[96] Alvarez had walked into a controversy older than Darwin's *The Origin of Species*.

Walter Alvarez, being a geologist, was more prepared for the controversy than his father. He recalled, "I knew geologists and paleontologists better than Dad did, and I was pretty sure there would be strong resistance."[97] It certainly became a heated debate in scientific journals; over two thousand articles were published on the impact hypothesis in the 1980s.[98] Given the popularity of dinosaurs, the debate entered the public discourse. As Walter would later point out, "Journalists thrive on hostile confrontations."[99] Even the editorial board of the *New York Times* got involved, proclaiming that

> Terrestrial events, like volcanic activity or changes in climate or sea level, are the most immediate possible causes of mass extinctions. Astronomers should

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[95] Niles Eldredge and Stephen Jay Gould, "Punctuated Equilibria: An Alternative to Phyletic Gradualism," in T. J. M. Schopf, ed., *Models in Paleobiology* (San Francisco: W. H. Freeman, 1972). The evolutionary biologist Ernst Mayr would disagree that Eldredge and Gould should get credit, citing his similar theory of 1954, but this is not the venue for that debate. Gould is most often associated with the theory and was its most vocal proponent.

[96] Gould, "Opus 200," *Natural History* 100, no. 8 (Aug. 1991), pp. 12-19. Cited in Shermer, *The Borderlands of Science*, p. 105.

[97] Walter Alvarez, *T. rex*, p. 78.

[98] William Glen, ed., *The Mass Extinction Debates: How Science Works in a Crisis* (Stanford: Stanford University Press, 1994), p. 58. Cited in Walter Alvarez, *T. rex*, p. 82.

[99] Walter Alvarez, *T. rex*, p. 85.

> leave to astrologers the task of seeking the cause of earthly events in the stars.[100]

Perhaps more galling is the implication of pseudo-science in the title of the editorial: "Miscasting the Dinosaur's Horoscope." Walter and Muller replied in a letter to the editor that there was much building evidence for their theory and concluded with: "May we suggest it might be best if editors left to scientists the task of adjudicating scientific questions?"[101] Yes, the uniformitarian paradigm was so entrenched that normally respectable but complete non-experts sounded off when Alvarez challenged it.

The impact theory had a great deal of evidence in its favor. Gradually, the tide turned towards universal acceptance. While it may have rankled the paleontologists for being counter to their founding paradigm, it was solid science. Alvarez's controversial work maintained falsifiability. Walter Alvarez pointed out that "One good set of connected [dinosaur] bones *above* the KT boundary would have offered strong evidence that the impact did not kill the dinosaurs, but none have been found."[102] Alvarez's team introduced new techniques into paleontology, drawing on the elder Alvarez's background in high-energy physics. While unconventional, this type of multidisciplinary work can be extremely fruitful. Gould said that Alvarez's mass extinction theory "could not have been … more exemplary for science."[103] In this case, outsiders brought a different perspective and different tools to bear on an existing question about the extinction of

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[100] *New York Times* editorial, "Miscasting the Dinosaur's Horoscope," 2 Apr. 1985.
[101] Richard Muller and Walter Alvarez, "Was it Nemesis That Killed the Dinosaurs," (letter to the editor) *New York Times*, 14 Apr. 1985.
[102] Walter Alvarez, *T. rex*, p. 87.
[103] Gould, *The Structure of Evolutionary Theory* (Cambridge, Mass.: The Belknap Press of Harvard University Press, 2002), p. 1306.

dinosaurs. The effects of the theory were twofold: Gould pointed out that "Alvarez's impact hypothesis forced paleontologists to acknowledge the potentially catastrophic nature of at least some mass extinctions."[104] Besides adding to our understanding of their fate, the impact theory changed our outlook on dinosaurs. Evolutionary biologists had been forced to believe that dinosaurs were uncompetitive; the early mammals were more fit. Alvarez's impact theory shows us that their 160 million year reign was no accident; their extinction was.

The dinosaur theory had many critics, but the mounting evidence overcame most objections. One criticism met a different fate. In 1985, Charles Officer of Dartmouth College presented a talk at the American Geophysical Union in San Francisco.[105] This followed up on a couple of articles he and Charles Drake published in *Science* criticizing one of the Alvarez team's claims. A massive asteroid impact would have produced immense heat that should have liquefied a lot of material, in this case sand. Walter's student Sandro Montanari found tiny glass spherules in Gubbio, Italy, that suggested a lot of sand was melted by the impact and distributed around the world. At the San Francisco talk, Officer said he had found these glass spherules everywhere they looked; they were not unique to the K-T boundary and so they were not evidence for the asteroid theory. Walter used his two-minute rebuttal time to point out that Officer and Drake had not cleaned their equipment properly. The spherules were insect eggs. "He showed that these spherules burned in air, didn't dissolve in acids, and could be dented if poked with a

[104] Gould, *Punctuated Equilibrium*, p. 261.
[105] Alvarez, *Adventures*, p. 265.

pin."[106] Luis described the reaction: "At that point the audience of several hundred earth scientists burst into laughter, something I'd never witnessed before in my fifty-three years of attending scientific meetings."[107] As any conspiracy theorist would tell you, being laughed at is the bane of the outsider. After warning his son and Muller that they could get laughed at, Alvarez was on the other side of the laughter, this one literal and public. As Gould said, the impact theory went "from a wild idea rejected out of hand by nearly all paleontologists in 1980, to a firmly documented virtual fact of nature by 2000."[108] The Alvarez team was now solidly in the scientific mainstream.

The Alvarez team's experience in paleontology provides an interesting example of the usefulness of outsiders. A physicist, a geologist, and two chemists revolutionized a fourth field, paleontology. As historian Lawrence Badash put it,

> no credible scientists in the 1970s would have linked the effects of nuclear weapons, particle microphysics, atmospheric chemistry, fire and smoke research, volcanic eruptions, ozone depletion, planetary studies, and dinosaur research.[109]

Walter Alvarez was aware that paleontologists would resent a group of outsiders upending their field; he knew his team challenged the gradualism paradigm and that they would meet with great resistance. He was a geologist, and while geology is distinct from paleontology, there is some overlap. Luis Alvarez was not aware that they would meet resentment as outsiders, but it could also be that he was accustomed to the type of deference a Nobel laureate received in his own field and from journal editors such as Phil

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[106] *ibid.*, p. 256.
[107] *ibid.*
[108] Gould, *The Structure of Evolutionary Theory*, p. 1304.
[109] Badash, *A Nuclear Winter's Tale*, p. 45.

Abelson. Alvarez's prestige provides an example of what sociologist Robert Merton called "The Matthew Effect"[110]: Luis Alvarez gets the credit because he is the senior team member. When writers cite "Alvarez, L., *et. al*.," they may be citing the first name alphabetically, but the elder Alvarez generally gets the most credit due to his name recognition. Richard Rhodes pointed out that Luis was very generous with authorship,[111] but other scientists are more likely to credit the scientist they recognize. The story of the Alvarez impact theory also provides an example of Michael Shermer's category of "Borderlands science": Muller's Nemesis hypothesis is arguably an overextension of the impact theory, but it is not nonscience. Like the SETI project searching for communication from extraterrestrial intelligence or Alvarez's search for monopoles in moon rocks, it has not yet succeeded and might never succeed, but it follows in the practice of sound science. It is a hypothesis; a scientist cannot be afraid to posit an idea that later turns out to be wrong.

[110] Robert Merton, "The Matthew Effect in Science," *Science* 159, no. 3810 (5 Jan. 1968), p. 56.
[111] Rhodes interview by author, 20 May 2009, p. 36.

Chapter 5: *Alvarez's Nutfile and the Boundaries of Science*

> It seems to me what is called for is an exquisite balance between two conflicting needs: the most skeptical scrutiny of all hypotheses that are served to us and at the same time a great openness to new ideas. If you are only skeptical, then no new ideas make it through to you. You never learn anything new. You become a crotchety old person convinced that nonsense is ruling the world. (There is, of course, much data to support you.)
>
> On the other hand, if you are open to the point of gullibility and have not an ounce of skeptical sense in you, then you cannot distinguish useful ideas from the worthless ones. If all ideas have equal validity then you are lost, because then it seems to me, no ideas have any validity at all.[1]
>
> - Carl Sagan, "The Burden of Skepticism," Pasadena lecture, 1987.

Alvarez was the ultimate insider in the community of American high-energy physics that he watched grow. He had trained with Arthur Holly Compton and Ernest Lawrence, worked at the Manhattan Project, was a full professor at the University of California, headed a large research group there, was widely published, and won a Nobel Prize. This kind of prestige attracted the attention of many who did not have such credibility. Alvarez kept a large "Nutfile," enough to tightly pack one archival box with over 1,200 pages at the Berkeley Bancroft library that keeps his papers. This messy collection of letters from those Alvarez considered to be outsiders is a bizarre and difficult collection to process: many letters are undated or signed with strange pseudonyms; most are unsorted, many are written in unreadably bad handwriting; it is

[1] Cited in Michael Shermer, *Why People Believe Weird Things: Pseudoscience, Superstition, and Other Confusions of Our Time* (New York: W.H. Freeman and Company, 1997), opening quote, p. *ix*.

sometimes difficult to tell when one letter ends and another starts. After our discussion of how the American high-energy physics community was created and how it defined itself, it will be useful to examine how the physics community has defined outsiders. In defining any community, it is often valuable to see how it defines the "other" and how others perceive the physics community. Linguist Noam Chomsky—an insider to academia but an outsider to physics—noted the prestige scientists possess and justified the barrier between them and outsiders:

> Look, in the physical sciences, there's by now a history of success, there's an accumulated record of achievement which simply is an intrinsic part of the field. You don't even have any right to enter the discussion unless you've mastered that. You could challenge it, it's not given by God, but nevertheless you have to at least understand it and understand why the theories have developed the way they have and what they're based on and so on. Otherwise, you're just not part of the discussion, and that's quite right.[2]

In other words, formal training is a barrier between the amateur and the physics insider. Furthermore, in Alvarez, we have an insider to physics who, as we saw, became known as an outsider to another field, paleontology.

To be clear, an outsider is very different than the type of nut in Alvarez's Nutfile. The difference between an insider and outsider is sociological; the difference between a non-nut and a nut is the difference between sense and nonsense. To give a clear example (but using only initials instead of names), one hand-written, twelve-page essay, from P.A.F. of Southhampton, England jumps right into physics theory and is the most radically incomprehensible letter in the collection, here reproduced with the original spelling. "Hyperation of the Atom" argues, among other things, that

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[2] James Peck, ed., *The Chomsky Reader* (New York: Pantheon Books, 1987), p. 16.

> The proton reaction in the core or sigma of the neutron, remendicates a primal issue of electrons which intense the parrameters in the sublimity of the scorch of burn of radiation fibers known as gamma frictions.[3]

Futhermore, P.A.F.'s theory hoped to produce practical results:

> To generate heat, two rods, equal of neutrons, viable in protons, must seminate at a distance, a transitional reflection in reverse of each other, thus causing the protude of one to disintegrate the other, causing friction by practitude.[4]

Unfortunately, several of P.A.F.'s terms are not words and no "radiation fibers" are "known as gamma frictions." Indeed, there does not appear to be anything that is widely known as a "gamma friction." This letter, one of the last in the Luis Walter Alvarez Papers, would epitomize the type of incoherent theories that Alvarez received throughout his career as a famous physicist. For all that has been written about the demarcation problem—what is and what is not science—this collection shows that the answer sometimes requires nothing more than common sense.

A particularly striking feature of Alvarez's career is that he seemed to have dozens of careers, so while he may have been an insider in physics, he could also be an outsider in other fields. He was a very successful inventor, particularly in optics. He was on the board of several companies, including Hewlett-Packard, and founded a company, Schwem Technologies, based on his optics work. His piloting career included over a thousand hours of flying and inventions like ground-controlled approach (GCA), which let pilots land in fog or under no visibility. These interests fit in the mold of a Richard

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[3] P.A.F. to Alvarez, Mar. 1986, LWAP, box 38, folder 5.
[4] *ibid*.

Feynman, the physicist so smart that he can master any field he tries. However, starting a business or patenting an invention hardly has insiders and outsiders.

For our purposes here, it is important to remember that Alvarez also "dabbled" in other fields where he could use his expertise in physics to solve problems that had confounded others. He knew what it was like to be an outsider, even if he was never a crank. The fact that Alvarez worked in so many disparate fields also helps explain why he had such a big Nutfile: as Alvarez wrote to a friend, "the only subject that has a worse collection of books about it (than the Kennedy assassination) is Egyptian pyramids. And I've had the dubious distinction of being heavily involved in both subjects."[5]

In one case, Alvarez's Nutfile contains several essays from a "nut" who seems to crossed the boundary. J. C. Cooper brought up some seemingly valid critiques of the work that earned Emilio Segrè the Nobel Prize. Cooper did not have the usual credentials, but he did get his critiques published in scientific journals and warranted a response by career physicists such as Allan Franklin of the University of Colorado at Boulder. Alvarez and Segrè had a bit of a feud and each privately challenged the other's Nobel work. It may have been tempting for Alvarez to side with Cooper, who asked for Alvarez's help and managed to build a case against Segrè. However, Alvarez realized the proper way to do things in the community of high-energy physics and stayed out of it. He was right; Cooper turned out to be a conventional "nut," complete with delusional prophesies and conspiracy theories. The physics community knows its borders well.

[5] Alvarez to Robert Shankland, 11 Feb. 1982, LWAP, box 40, folder "S 1978-87 [2 of 3]."

THE NUTFILE

We have one empirical definition of a nut: Alvarez received a letter and sorted it under his "Nutfile." He did not leave a methodology or rating system to help us understand what he meant by a nut, but a look through the file suggests he was including the obvious nuts who made no sense or had little basic understanding of mathematics or physics. That he included creationists and people who did not believe in Einstein's theory of relativity says a bit about the science insider vs. outsider. He lumped conspiracy theorists in with people whose spelling and grammar suggested a very minimal education. When one reads through Alvarez's Nutfile, one can occasionally find a letter that seems reasonable until something tips the reader off to that obvious reason Alvarez filed it there. So there is seemingly no mystery, nothing to solve.

Yet Alvarez found his Nutfile interesting and generally replied to even the craziest of letters. Alvarez mentioned his Nutfile to biographer Richard Rhodes, who reported that Alvarez had "formulated a sort of standard response." Rhodes continued: "He told me that he had figured out a way to answer them that usually made them happy and shut them up. It had to do with something like, 'You never know, science is wide open.'"[6] Rhodes appeared in one letter from D.L.E. of Mission, Kansas. Rhodes had forwarded D.L.E.'s letter to Alvarez, who dodged the question by telling D.L.E. that he was an experimentalist, not a theorist.[7] Alvarez was polite and respectful. Alvarez's replies to the nuts is usually not included in the Nutfile, instead, he would often write a

[6] Richard Rhodes interview by author, 20 May 2009, p. 23.
[7] Alvarez to D.L.E., Mission, Kansas, 6 Jun. 1986, LWAP, box 38, folder 4.

handwritten note to his secretary on the incoming letter instructing her to reply. Sometimes the poor writing and focus in the letter could give Alvarez an out. In one letter, Alvarez replied, "Thank you very much for your long and interesting letter. I've read it carefully, three times, but I can't learn what it is that you'd like me to do."[8] He did not see someone's ignorance as an excuse for poor manners.

Perhaps Alvarez thought he had no choice but to answer nut letters. One letter raised his concern. A letter dated 26 July 1965 and signed only "Pero" ominously suggests: "Suggest you look at Great Pyramid and Sphynx. Good Luck."[9] Besides a return address in Chicago, that is all the letter says. The timing is odd, since Alvarez only seriously discussed his pyramid project in 1964, built most of the equipment in 1966, and published his results in 1968. Either Pero knew about the project very early on, or, more likely, he was a nut who happened to write to Alvarez at the right time.[10] We know that Alvarez took this letter seriously because of the presence of another letter in the same file from Robert C. Hennecke, Assistant Security Manager at the Lawrence Radiation Lab, indicating that they were unable to track down Pero.[11] The letter from the security agent says the address led them to a P.A.J. of Chicago and reveals no insight that Pero could be a Spanish nickname, a misspelling of "perro," meaning dog. "Pero" does translate as the conjunction "but," although that seems less likely a nickname. Hennecke seems to think the letter is not too serious, saying they failed to find Pero "short of a full-blown

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[8] Alvarez to W.H.M., Oakland, CA, 6 Apr. 1987, LWAP, box 38, folder 4.

[9] Pero, Chicago, IL to Alvarez, 26 July 1965, LWAP, box 38, folder 1.

[10] The timeline is derived from Alvarez, *Alvarez: Adventures of a Physicist*, (New York: Basic Books, 1987), 230-236.

[11] Robert C. Hennecke, Assistant Security Manager, Lawrence Radiation Laboratory to Alvarez, 17 Aug 1965, LWAP, box 38, folder 1.

investigation."[12] A more specific threat would probably have gotten such an investigation, but not such an incredibly vague letter.

Alvarez wrote to a friend, philanthropist William T. Golden, that he feared that some of the nuts were dangerous. Conspiracy theories play a large role in the thinking of many nuts. Alvarez told Golden this story: "One of these people appeared one time in the office of a friend of mine; when my friend's secretary said he was out, the caller naturally assumed that the secretary was part of the conspiracy, and shot her through her heart right then and there."[13] Perhaps Alvarez replied carefully to the nuts out of concern for his safety. It is not difficult to imagine that the nuts might be unstable. While Alvarez did his best to answer all letters, some were beyond his patience. One letter in the nutfile came not from a nut, but from a translation service. Addis Translation International of Woodside, CA wrote to Alvarez about a letter written in Spanish:

> The writer of the letter is self-admittedly not a scientist, and does not write Spanish very well either. If Dr. Alvarez is interested in amateur philosophies of science, we shall be glad to translate all of this material.[14]

So not every letter got a reply, or at least the Alvarez Papers do not contain a request to translate this difficult letter.

No matter what he thought of the stability of the nuts, Alvarez did not concede any ideas to them. D.A. wrote to Alvarez to share the work of psychic Edgar Cayce in relation to Alvarez's pyramid project. Alvarez replied somewhat sarcastically, "I very

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[12] *ibid*.

[13] Alvarez to William T. Golden 27 Feb. 1967, LWAP, box 44, folder "William T. Golden." Cited in Jason Linn, "Not Suffering Fools Gladly: The Publicity and Patronage of Luis Alvarez's Pyramid Project," p. 29. Unpublished manuscript. Personal communication, 2 Jun. 2011.

[14] Addis Translations International to Ms. Smith (Alvarez's secretary) 27 Jan. 1969, LWAP, box 38, folder 2.

much enjoy reading fiction, but I'm sorry to say that I really do not enjoy what appears to me to be fiction, when it is dignified with the external trappings that give it the appearance of history or science."[15] So Alvarez would reply to the nuts, sometimes sarcastically, but always honestly.

The previous letter raises an interesting question: how did Alvarez decide what went in the Nutfile? The letter from D.A. was not in the Nutfile, despite being about a psychic who could tell Alvarez how to find treasure in the Egyptian pyramids. Instead, some letters about the pyramids that were clearly from nuts went in the Pyramid file, under "Miscellaneous." One letter from a Jim Manzari made its way into the general correspondence folder, "M 1946-60 [1 of 2]." Manzari wanted Alvarez's advice on his work with an x-ray tube he had found: he wanted to study "the killing and mutating effects of x-radiation."[16] Alvarez replied that he should be careful but that he go for it![17] Alvarez always had a streak of the danger seeker. He described adventures in his youth exploring construction work, "usually by sneaking past the guards in the middle of the night. We climbed the three-hundred-foot clinic tower when it was only a skeleton of steel beams. We explored the power house and scaled the inside of its two-hundred-foot brick smokestack."[18] He argued that "a controlled disrespect for authority is essential to a scientist," who should have "an intense curiosity that no Keep Out sign could mute."[19]

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[15] Alvarez to D.A. 30 Jan. 1967, LWAP, box 32, folder "Miscellaneous 1966-68." Cited in Linn, "Not Suffering Fools Gladly," p. 15.
[16] Jim Manzari to Alvarez 28 Oct. 1958, box 3, folder "M 1946-60 [1 of 2]." I did not use initials here because there is no implication that Alvarez thought he was a nut.
[17] Alvarez to Jim Manzari 5 Nov. 1958, box 3, folder "M 1946-60 [1 of 2]."
[18] Luis Alvarez, *Alvarez: Adventures of a Physicist* (New York: Basic Books, 1987), p. 14.
[19] *ibid*.

Perhaps Manzari's scientific curiosity is why Alvarez did not put his letter in the Nutfile. Perhaps Manzari was not a nut because he was not espousing any non-science or challenging mainstream physics, just conducting amateur science experiments. Perhaps the decision to put something in the Nutfile was not always clear cut.

Letters asking Alvarez to be in a "Hispanics in science" list did not go in the Nutfile, even if Alvarez thought they were misguided, as we shall see in chapter six. Some nut letters were sorted by category, like the pyramid letters that went in the pyramid file, while other pyramid letters, like the one from "Pero," did go in the Nutfile. Most were not from academics, but a small handful were, such as a letter from J.A.L., who was a faculty member in the department of physics at the National Technological University in Córdoba, Argentina. J.A.L. sent Alvarez a book titled, *Self-Propulsion: A New Flying Technique*, a seemingly nutty idea that Alvarez filed with the creationists and psychics.[20] Instead of guessing at Alvarez's decision process, or even the likely possibility that he simply did not put much thought into the Nutfile, we can look at what philosophers and historians of science have said about defining the nut and defining science.

HOW DO YOU SPOT A NUT?

To derive meaning from the Nutfile, we have to look at some definitions of a nut. Not surprisingly, historians of science have not dedicated much time to the nut, instead writing about outsiders in science that can be distinguished from nuts in that outsiders come off as reasonable. The best two works on nuts come from physicists. Jeremy

[20] J.A.L. to Luis Alvarez 1959, LWAP, box 38, folder 1.

Bernstein of the Stevens Institute of Technology and science writer for the *New Yorker* wrote an accessible, popular essay on how to distinguish a nut from an unknown writer with a potentially good idea. John Baez of the University of California, Riverside devised a system for ranking nuts. Bernstein borrowed heavily from the philosophy of science while Baez worked from experience with nuts. We shall discuss them at length below.

Many of the authors whose letters are in the Nutfile had strong opinions on the demarcation problem. One letter from B.M. of Phoenix asked Alvarez for a critique of his “Bicorpuscular-Photon Hypothesis.” His paper was accompanied by a handwritten letter almost challenging Alvarez to disprove his work:

> The attached hypothesis must be refuted clearly and unequivocally.
>
> Would you please list reasons, based on established scientific <u>facts</u>, <u>observations</u>, or <u>laws</u>, which may be used in refutation?
>
> Please avoid interpretations, theories, hypotheses, "principles," personal biases, opinions, or authoritarianism. These cannot be built into a valid refutation. (For example, in the "Compton Effect," the <u>interpretation</u> has far overshadowed the <u>observations</u> on which it is based. Please use only the <u>observations</u> in refutation, <u>not</u> the <u>interpretation</u> of these observations, regardless of how many scientists subscribe there to. A <u>poll</u> of scientists is worthless to me in refutation.)[21]

B.M. seems to think Alvarez would use a sociological test of the demarcation problem. “A poll of scientists” implies that a scientist’s insider status by means of a Ph.D. and a faculty position allows him or her to decide what is science by a popularity contest. Arguably, a poll of scientists would be a very good test. The National Center for Science

[21] B.M. to Alvarez, 19 Aug. 1968, LWAP, box 38, folder 2.

Education has countered lists of scientists who doubt evolution with its Project Steve.[22] While a poll of all scientists would be difficult to conduct, Project Steve narrowed the field by asking only for signatures from scientists named Steve or some variant of that in honor of Stephen Jay Gould. The 1,166 Steves who have signed far outweigh the few hundred signatures of scientists who deny evolution. The nut would like to think that, in this American democracy, a man more right than all the scientists is Thoreau's "majority of one."[23] Science does not work that way and it cannot; how could science progress if it were so mired in self-doubt?

Other letters in the Nutfile reflect the writers' sense that they are outsiders to science but should be given a fair hearing. An anesthesiologist from Los Angeles, Dr. G.J.P., wrote to Alvarez about his theory on the atomic nucleus. He opened one of a series of letters thus:

> I have had no training in Physics since junior college days in 1922. I have read somewhat in Science magazine of the AAAS and in a few books, but am not closely in touch with developments in nuclear physics. I hope to be forgiven for the unsophistication shown in my writing.[24]

This type of disclaimer is common, although many are confrontational and many more do not seem to be aware that having no track record as a scientist could be a hindrance. The solutions to the demarcation problem found in Alvarez's Nutfile provide one way to identify a nut. If the writer thinks the demarcation problem is no problem at all or perhaps just a way for scientists to impose their will, then he is probably a nut.

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[22] http://ncse.com/taking-action/project-steve. Retrieved 26 Jun. 2011.

[23] Henry David Thoreau, *Civil Disobedience and Other Essays* (New York: Dover Publications, 1993), p. 8.

[24] G.J.P. to Alvarez, 14 Oct. 1968, LWAP, box 38, folder 2.

Philosophers have proposed numerous solutions to the demarcation problem. Karl Popper's falsifiability criterion is perhaps the most well known, but it has largely fallen out of favor with philosophers of science. Physicists Alan Sokal and Jean Bricmont argue that practicing scientists do not concern themselves with falsifiability, so it is not a realistic analysis of how science actually works.[25] We shall discuss falsifiability in relation to Bernstein below. The fact of the matter is that science is difficult to define. 1960 Nobel Laureate Sir Peter Medawar observed:

> Ask a scientist what he conceives the scientific method to be and he will adopt an expression that is at once solemn and shifty-eyed: solemn, because he feels he ought to declare an opinion; shifty-eyed, because he is wondering how to conceal the fact that he has no opinion to declare.[26]

The French historian of science Georges Canguilhem argued that a scientific ideology is an explanatory system that must emerge from another, established field that is adjacent to its field of study.[27] Darwinian evolution relied on Lyell's geology; Newtonian mechanics emerged from the astronomical works of Galileo and Kepler. Michel Foucault cleverly—and subtly—argued in *Madness and Civilization* that psychoanalysis is not a science because it does not have a foundation in an established science.[28] Without such a foundation, a discipline is free-floating, unable to be confirmed or denied by other scientists. In a related vein, Thomas Kuhn's concept of a scientific paradigm necessitates

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[25] Alan Sokal and Jean Bricmont, *Fashionable Nonsense: Postmodern Intellectuals' Abuse of Science* (New York: Picador, 1998).

[26] Peter Medawar, *Induction and Intuition in Scientific Thought* (Philadelphia: American Philosophical Society, 1969), p. 11. Cited in Shermer, *Why People Believe Weird Things*, p. 18.

[27] Georges Canguilhem, "What Is a Scientific Ideology?" in *Ideology and Rationality in the History of the Life Sciences* (Cambridge, Mass.: The MIT Press, 1988), p. 38.

[28] Michel Foucault, *Madness and Civilization: A History of Insanity in the Age of Reason* (New York: Vintage Books, 1988). As is typical of a work by Foucault, there are more than one interpretation, none of them obvious.

that a science have a founding paradigm: "anyone examining a survey of physical optics before Newton may well conclude that, though the field's practitioners were scientists, the net result of their activity was something less than science."[29] Perhaps a science can be defined as an explanatory system with a founding paradigm grounded in another, established field. However, for all the work of the philosophers of science, these definitions are difficult to apply to the nut as gauged by a single letter. Instead of theory, we need practical tools to define the nut.

Historians of science have created a sizeable literature on science outsiders, those people on the fringes of science that sometimes distract from what the writer believes is the true goal of science. Other writers defend the iconoclast that is original enough to innovate when others are stuck in a Kuhnian paradigm. Many writers have discussed the insider-outsider tension in the context of science professionalization, a process that happened well before American physicists developed an identity. American science arguably professionalized in the mid nineteenth century as described in historian Sally Gregory Kohlstedt's *The Formation of the American Scientific Community: The American Association for the Advancement of Science 1848-60*.[30] The need for the A.A.A.S. developed out of a failure to clearly define science by earlier groups such as the American Philosophical Society and the American Academy of Arts and Sciences in the mid-nineteenth century. By comparison, England's Royal Society dates back to 1660 and

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[29] Thomas Kuhn, *The Structure of Scientific Revolutions, Third Edition* (Chicago: The University of Chicago Press, 1996), p. 13.
[30] Sally Gregory Kohlstedt, *The Formation of the American Scientific Community: the American Association for the Advancement of Science 1848-60* (Urbana: University of Illinois Press, 1976).

the British Association for the Advancement of Science was founded in 1831. In that context, amateur scientists were an embarrassment to American scientists because they were able to publish their curiosities and even nonsense alongside the work of professional scientists.[31] This is a particularly important issue in a democracy, where government funding for science must maintain scientific standards, yet distribute money at least in part to satisfy constituents and taxpayers. Government funding for science in the United States means the interests of the people come to bear on the issue of demarcation. It is safe to say that by Alvarez's time, physicists were not concerned that nuts were going to publish in respectable journals, so Kohlstedt's approach does not translate well to this work. The same could be said of John D. Holmfeld's work on outsider theories in geology at the 1853 meeting of the A.A.A.S.[32] It is notable that the possibility of a journal publishing an article by a nut remained, as we shall see in the unlikely case of J.C. Cooper.

An article by Arthur Molella on Joseph Henry of the Smithsonian Institution seems like a sure fit, but suffers the same problems for our purposes as Kohlstedt and Holmfeld. Henry was the first Secretary of the Smithsonian Institution from 1846-1878 and received a great many letters from what he called "visionary theorizers," or Alvarez would call nuts. Since the Smithsonian is a government institution, Henry thought he had a responsibility to the people of the United States, and took care to answer "virtually all

[31] *ibid*., p. 21.
[32] John Holmfeld, "From Amateurs to Professionals in American Science: the Controversy over the Proceedings of an 1853 Scientific Meeting," *Proceedings of the American Philosophical Society* 114, no. 1 (Feb 16, 1970), pp. 22-36.

letters received: to monitor the 'immense mental activity which existed in this country in regard to scientific speculations,' to document the topics of greatest interest, and to instill in his correspondents a respect for scientific standards."[33] Thus Henry found himself the arbiter of what is scientific, the doorman of the demarcation problem. His correspondence with the nuts took on extra weight since he thought Europeans would judge American science by the ideas floating around in this country. He was further concerned that the public would not be able to tell the difference between science and the visionary theorizers. He maintained that the scientific discipline should have a "monopoly on truth."[34] He worked towards a definition of science, but, Molella argues, decided a letter's worth partly sociologically: "Henry looked for signs of deference to those eminent forebears and the values they stood for."[35] However, like other works on nineteenth century quacks, it does not translate well to Alvarez's career. As Molella said, "The societal organization of American science in Henry's era was still ill-formed."[36] While there are interesting parallels, the nut in the twentieth century is another beast.

Physicist and journalist Jeremy Bernstein wondered, if we are so sure that we can identify a letter from a crank, then, as the title of his essay asks, "How Can We Be Sure That Albert Einstein Was Not a Crank?"[37] In one of two primary approaches to defining a nut, Bernstein wonders how the theory of relativity would have sounded coming from

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[33] Arthur P. Molella, "At the Edge of Science: Joseph Henry, 'Visionary Theorizers,' and the Smithsonian Institution," *Annals of Science* 41, no. 5 (1 Sep. 1984), p. 458.
[34] *ibid*, p. 448.
[35] *ibid*, p. 458.
[36] *ibid*, p. 460.
[37] Jeremy Bernstein, "How Can We Be Sure That Albert Einstein Was Not a Crank?" in *Cranks, Quarks, and the Cosmos* (New York: Basic Books, 1993), pp. 15-27.

an unknown patent office clerk by the name of Albert Einstein. After all, Einstein did not have a professorship or even his doctorate yet when he submitted his special theory of relativity to the German physics journal *Annalen der Physik* in 1905. Bernstein put himself in the shoes of a physics professor receiving a letter, which, "using totally unfamiliar kinds of reasoning," argues that "essentially all of the physics I have been teaching is wrong. Not just wrong in a few minor details, but fundamentally wrong."[38] Bernstein takes a philosophical, even Cartesian approach to the question of what makes a nut or crank. Ultimately, his definition combines the work of Danish physicist Niels Bohr and Austrian-British philosopher of science Karl Popper. Bernstein's is a deductive approach, relying on the philosophy of science to let us weed out a potentially valid revolutionary theory from the hundreds of insane ones.

Bernstein proposed two tests to see if a theory is scientific: correspondence and predictiveness. Correspondence is a term associated with Danish physicist and early proponent of quantum mechanics, Niels Bohr.[39] Bohr's work came at a time when physics was being radically transformed by Max Planck's discovery that energy is quantized, that is to say, that energy can only be exchanged in specific quantities, not in any arbitrary levels. Quantum mechanics predicted strange behaviors for particles small enough to be affected by this minimum energy level. A single photon takes several different paths simultaneously; an electron "tunnels" through a barrier without ever being

[38] *ibid*., p. 15.

[39] Bohr's correspondence principle develops in a series of papers and correspondence large enough that it comprises the entire third volume of his collected works. Niels Bohr, *Collected Works: Volume 3 The Correspondence Principle (1918-1923)* (Amsterdam: North-Holland Publishing company, 2008).

in the barrier; particles behave like waves and waves behave like particles. Surely these effects are so preposterous as to disprove quantum mechanics.[40] Bohr's response was essentially that we should test quantum mechanics in cases we understand. We know what a ball does when we throw it. What does quantum mechanics say? The ball is so big that energy levels are indistinguishable from a continuous (non-quantized) ball. In other words, the weirdness of quantum mechanics disappears at human scale. The same could be said of the theory of relativity—at everyday speeds, the effects of relativity are essentially nonexistent. This is correspondence: if a new theory makes outrageous predictions in special cases, check to see what the theory says about phenomena that we understand. A new theory should be compatible with old theories in cases where the old theory works.

Bernstein's second test is predictiveness. This is easier to understand. A theory should make specific and testable predictions. If it does not make any predictions, then it should not be part of science. If it makes predictions that cannot be checked, it is not useful science. If it makes predictions that experiment prove to be wrong, then the theory may be scientific, but it is wrong. This test is fundamental to the scientific method going back to the Enlightenment, but it is probably best elaborated by Karl Popper's concept of falsifiability.[41] Popper argued that to be a part of science, a theory has to be testable and,

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[40] Physicist Erwin Schrödinger famously described the strange properties of quantum mechanics with the thought experiment known as "Schrödinger's Cat." He hypothesized about a box that contains a cat and a vial of poison gas triggered by an atomic reaction that is fifty percent likely to break the vial. By the Copenhagen Interpretation of quantum mechanics that Bohr championed, the cat would be both dead and alive until someone looked in the box. At that instant, the cat would become alive or dead.

[41] Karl Popper, "The Demarcation Between Science and Metaphysics," in *Conjectures and Refutations: The Growth of Scientific Knowledge* (London: Routledge, 1963). Popper first wrote about the demarcation

more to the point, it must be possible to prove the theory wrong. If it is not possible to prove a statement wrong, it might still be correct, but it is not science; it cannot participate in the scientific method. Einstein's fame outside physics circles came not when he published his very convincing special theory of relativity in 1905 or his General Theory in 1916, but when British astrophysicist Arthur Eddington confirmed the predicted bending of light by the sun's gravity in 1919. Quantum mechanics predicts a multitude of strange behaviors, but anybody who has used transistors or lasers can confirm that, as the saying goes, if it happens, then it must be possible.

Bernstein agreed with Baez and Alvarez that the use of a lot of capital letters and exclamation points is a good sign of a nut, but he offers some interpretation as well. He notes that every nut letter he has seen is from a man. Alvarez's file is largely consistent with that, with the possible exception of a letter from a "Nona," as nearly as I could determine from the handwriting.[42] Nona certainly sounds like a woman's name, and the worst of the handwriting is on the last name, not the first, but it is still such a rare exception that Bernstein is essentially correct that writing crank letters to scientists is a man's hobby. However, during Alvarez's career, professional physics was also mostly a man's field.

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issue in *The Logic of Scientific Discovery* (New York: Harper & Row, 1959). The original German version was published in 1934 as *Logik der Forschung*. Unfortunately, Popper's writing is nearly impenetrable to the non-philosopher and is best understood in commentaries.

[42] Nona, Bellevue, WA to Alvarez, 29 May 1985, LWAP, box 38, folder 4. Her letter is discussed below in the section "Dinosaur Work Attracts Nuts." Since I discuss the gender of her first name, she will be Nona without middle or last initials.

Bernstein quoted physicist Wolfgang Pauli on bad science: "there were physics papers so bad they were not *even* wrong."[43] Bernstein said what Pauli described is beyond bad science, "It is as if the crank is speaking in tongues."[44] The bad science Pauli discussed is at least written in the language of modern physics: higher mathematics. To the frustration of science popularizers, much is lost in the translation when physics is explained in non-mathematical terms. Without that foundation, the nut has little chance of success.

Where Bernstein may overreach is his assertion that "The crank is a scientific solipsist who lives in his own little world. He has no understanding or appreciation of the scientific matrix in which his work is embedded."[45] Of course the nut lives in his own little world in the sense that he is not part of the scientific community, of course he has no appreciation of the world of science as a matrix of interconnected ideas, but I disagree that he does not understand. As we shall see, the nuts in Alvarez's Nutfile seem to be intensely aware of their status as outsiders. Bernstein adds that "the authors of these papers do not want to be instructed: they want to be *affirmed*."[46] This much is true. Bernstein's point is that the nut wants to play in the big leagues of science without the years of training. My only disagreement is that many probably wish they could have gone through the training. They almost certainly would not have passed, but how do we know that they would not have liked to try?

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[43] Bernstein, *Cranks*, p. 27.
[44] *ibid*.
[45] *ibid*., p. 20.
[46] *ibid*., p. 27.

Besides Bernstein's tests, there is another approach that is inductive, that is, by example. Physicist John Baez of the University of California, Riverside has seen enough letters from nuts to know one when he sees it, like a Supreme Court justice defining pornography. Baez maintains a humorous website with a "Crackpot Index," which he describes as "A simple method for rating potentially revolutionary contributions to physics."[47] In effect, Baez's Crackpot Index gives a point score to an oddball letter to define how much of a crackpot the author is. It is a list of thirty-seven characteristics that help identify a nut and rank them by nuttiness. Points are awarded for such things as writing in all capital letters or misspelling "Einstein." Some of the entries suggest that the rating system is not meant for single letters but for a person's pseudoscientific career, but it should suffice in this context even though Alvarez's Nutfile rarely has more than one letter from any individual. Baez almost certainly wrote the Crackpot Index for fun. Besides being funny, it is not what physicists would call "normalized," that is, there is no maximum score. A person or letter can keep racking up points without limit. For example, a letter in all capital letters gets five points for each word. It seems unlikely that a letter twice as long should almost necessarily be twice as cracked.

Baez has come to recognize terms commonly used by the nut like a reverse shibboleth that exposes a letter using terms like "only a theory," "paradigm shift," "hidebound reactionary," or "self-appointed defender of the orthodoxy."[48] His eighth item grants five points for "each mention of 'Einstien,' 'Hawkins' or 'Feynmann,'" then

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[47] Baez Crackpot Index, http://math.ucr.edu/home/baez/crackpot.html. Retrieved 21 April 2011.

[48] *ibid*., numbers 16, 19, 27, and 28.

grants twenty points to anybody who writes to him “saying that I misspelled ‘Einstein’ in item 8.”[49] Some are common sense, like ten points “for mailing your theory to someone you don’t know and asking them not to tell anyone else about it, for fear that your ideas will be stolen.”[50] Some are obvious warning signs, like his warning against any letter that uses “science fiction works or myths as if they were fact.”[51] Baez grants twenty points for that and forty points “for claiming that the ‘scientific establishment’ is engaged in a ‘conspiracy’ to prevent your work from gaining its well-deserved fame.”[52] He grants ten points for comparing yourself to Einstein, twenty for Newton, and forty for Galileo because the last comparison suggests “that a modern-day Inquisition is hard at work on your case.”[53] This tendency towards conspiracy theory implies that the nut is aware of his exclusion in the field of professional science and is resentful of it. However, this does not excuse the nut who invariably turns to persecution to explain his position instead of his own lack of understanding of mathematics and physics.

Baez’s list, like Bernstein’s complaints about nuts “speaking in tongues,” also identifies many ways that the nut misunderstands how science works. He awards ten points for saying something like “I’m not good at math, but my theory is conceptually right, so all I need is for someone to express it in terms of equations.”[54] That is like saying “I’m very good at Russian poetry, I just need someone to translate my work into

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[49] *ibid.*, numbers 8 and 20.
[50] *ibid.*, number 12.
[51] *ibid.*, number 23.
[52] *ibid.*, number 34.
[53] *ibid.*, numbers 18, 22, and 35.
[54] *ibid.*, number 15.

Russian." It reflects a deep misunderstanding of how interconnected physics is to mathematics. Baez's list further backs up Bernstein's second rule on predictiveness. Baez's final item is "for claiming you have a revolutionary theory but giving no concrete testable predictions."[55] In a nod to the importance of testability, this item earns fifty points, the highest single item on the list. Here Baez and Bernstein agree that a theory must produce results and those results must be within the domain of experimental science. This is one of the reasons Intelligent Design is not science. "God wanted it that way" provides no testable predictions except for the heretic who believes he knows God's will.

The literature on outsiders is also filled with stories of the lone scientist working against the mainstream, only to be proven right in the end. How do we square that with the dominant pattern that a nut is a nut? For one, we have to distinguish the scientifically legitimate outsider from the nut. Zoologist Stephen Jay Gould wrote about a few of his heroes in his excellent *The Panda's Thumb*.[56] He gives the example of Chicago geologist J Harlen Bretz and his theory on the channeled scablands of Washington State.[57] Bretz argued around 1920 that the system of canyons must have been formed by a catastrophic flood and not by the generally assumed gradual erosion. As Gould reports, orthodox geologists "held firm to the dogma that catastrophic causes must never be invoked so

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[55] *ibid.*, number 37.

[56] Stephen Jay Gould, *The Panda's Thumb: More Reflections in Natural History* (New York: W. W. Norton & Company, 1992).

[57] Stephen Jay Gould, "The Great Scablands Debate," in *The Panda's Thumb*, pp. 204-216. Note that there is no period after the "J" in J Harlen Bretz.

long as any gradualist alternative existed."[58] Gould further criticizes the dominant gradualists: Bretz "stood against a firm, highly restrictive dogma that never had made any sense: the emperor had been naked for a century."[59] Calling the mainstream interpretation part of a "dogma" feeds into the nut's persecution complex, but it turns out that catastrophic explanations have become well accepted through the work of scientists like Gould and in part through Alvarez's theory of catastrophic dinosaur extinction. Could it be that the outsider is sometimes right?

As we shall see, Alvarez was an outsider in many fields, but he was not a nut in any of these cases. Paleontologists' resistance to his dinosaur extinction theory is the clearest example, but he was also an outsider to pyramidology and forensics. His work in those areas would not score anything on Baez's scale. His papers on those topics would pass Bernstein's tests with flying colors in that they are repeatable, although admittedly, his pyramid work has not been confirmed by other tests due to the immense difficulty of probing the massive structures.

Alvarez discussed a scientist who straddled the border between insider and outsider. While a graduate student at the University of Chicago, Alvarez learned of the discovery of the neutron at a speech by Chicago chemist William Draper Harkins. In Alvarez's words, "Harkins had predicted the neutron back in 1915 or 1919 and it had finally been discovered by some young upstart named Chadwick,"[60] implying that

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[58] *ibid.*, p. 197.
[59] *ibid.*, p. 201.
[60] Charles Weiner and Barry Richman interview with Alvarez on 14 February 1967, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, Session I, p. 12.

Harkins did not deserve that credit. Nobel Laureate in chemistry Robert S. Mulliken presented a very different image of Harkins in his Biographical Memoir. Mulliken argued that "during the period 1913-1928, Harkins and his students were the only Americans engaged in work relating to the structure of the atomic nucleus."[61] Mulliken argues that Harkin's work on the nucleus was central to the discovery of the neutron, which he believed was a combination of a proton and an electron (he was close). Mulliken continued: "In 1921, in a paper communicated to *Philosophical Magazine* by [Cambridge physicist Ernest] Rutherford, Harkins first introduced the word *neutron* to describe this particle."[62] He admits in a footnote that "the *word* neutron was first used by W. Sutherland in 1899 and in 1903 by W. Nernst, but for different concepts than that of the current neutron."[63] However, he strengthens his claim about Harkins: "Harkins in an article in *Science* in 1946 said 'that a neutron exists was assumed by Rutherford and Harkins in 1920.'"[64] It is somewhat odd to cite a 1946 article by Harkins as proof that Harkins predicted the neutron in 1920.

Alvarez had a different and much harsher opinion of Harkins:

> In fact as graduate students, we always used to say, "Well, let's harkin' back to 1915." Harkins was a chemist who had apparently written some articles on nuclear structure in which he predicted everything, and anytime anything new was found you could always go back and find it in Harkins's articles. It was better for Harkins if you didn't go back and look because you then also found that he predicted everything. It was bound to show up. He never got any recognition by any physicist and, until his dying days, he thought he should

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[61] Robert S. Mulliken, *William Draper Harkins 1873-1951: A Biographical Memoir* (Washington, D.C.: National Academy of Sciences, 1975).
[62] *ibid*., p. 59. Emphasis in original.
[63] *ibid*. Emphasis in original.
[64] *ibid*.

> have been recognized for a number of discoveries in physics, in view of the fact that he predicted everything with a shotgun approach. Nobody gave him any credit and it really hurt him.[65]

Alvarez framed Harkins as an outsider within the walls of the prestigious University of Chicago. Of course, this may be a case of departmental animosity, since Harkins was in chemistry and Alvarez was in physics. However, physics and chemistry overlapped a bit in the early days of nuclear physics. To give one example, Alvarez's friend Glenn Seaborg was a nuclear chemist, a job description that belies the intersection of nuclear physics and chemistry in the mid-twentieth century; chemists usually work on the electron shells, not the nucleus. Seaborg won a Nobel Prize in chemistry and became the Chancellor of the University of California at Berkeley and Chairman of the Atomic Energy Commission. Discovering a new element is in the realm of chemistry, but since the early days of high-energy physics, it is done at a particle accelerator. Either way, Alvarez's description of Harkins was harsh:

> He got to be pretty much of a bore. I saw him buttonholing people in the National Academy corridors, telling them how he discovered things and people tried to get away. This was a tragic sort of thing. He really thought he'd done something and he hadn't done anything at all—in that area.[66]

So was Harkins an outsider? Perhaps, but he was not a nut. Again, Baez's rating system would surely give him a low score. He may have claimed to have discovered something he did not discover, but he didn't blame a conspiracy or, like Baez described in item

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65 Charles Weiner and Barry Richman interview with Alvarez, 14 February 1967 (Session I), p. 12.
66 *ibid*.

thirty one, claim that his "theories were developed by an extraterrestrial civilization."[67] No, the nut is a different phenomenon than the outsider.

One must be careful with a blanket use of the word "nut." Alvarez also used the term to describe Alfred Loomis, whom he called a "time nut."[68] However, this is an entirely different sense on "nut." Loomis was in some ways more of an insider than Alvarez.[69] Loomis was a wealthy lawyer and investor with an intense interest in science, particularly physics, whom Alvarez called "the last of the great amateurs of science."[70] "He became a close lifelong friend and a second father to"[71] Alvarez, and helped Ernest Lawrence get funding for his cyclotron work in the 1930s. Alvarez described Loomis's position in physics in the 1930s to the 1950s: "The rest of us were sort of workers in the fields, and we didn't come into contact with the kinds of things that Ernest Lawrence and Alfred Loomis and other people were thinking about in those days."[72] In the days before the Manhattan Project and seemingly unlimited federal money and resources for physics, Loomis was a helpful philanthropist to have around, even if he did so as a hobby after retiring from Wall Street. So why call him a "time nut"? Alvarez described Loomis's interest in accurate time keeping:

> If you see him today, he always wears two Accutrons, one on his right wrist and one on his left wrist, and he checks them every day against WWV [a shortwave radio broadcast that tells the time], and if one is gaining a half second on the other he will wear it on the outside of his wrist instead of the

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67 Baez Crackpot Index, number 31.
68 Charles Weiner and Barry Richman interview with Alvarez, 15 February 1967 (Session II), p. 19.
69 For more on Loomis, see Jennet Conant, *Tuxedo Park: A Wall Street Tycoon and the Secret Palace of Science that Changed the Course of World War II* (New York: Simon & Schuster, 2002).
70 Alvarez, *Adventures*, p. 79.
71 *ibid*.
72 Alvarez interview by Charles Weiner and Barry Richman 15 Feb. 1967, session II, p. 20.

> inside so that gravity changes the rate of the tuning fork, and the two watches track each other, and WWV, to within less than a second a day.[73]

In this case, "nut" means enthusiast, but there are certainly no letters from Loomis in Alvarez's Nutfile. Alfred Loomis was the rare combination of an outsider—in that he did not have a Ph.D. in physics—and an insider—in that he was better connected than any of the physicists with whom he worked.

Diving into a collection like Alvarez's Nutfile offers, above all, an opportunity to define the "Nut" and thus help define the insider. The existing literature includes some discussion on defining science, which will serve as a useful backdrop, but in this context, the individual and his specific ideas are in question, not an entire program such as Intelligent Design. Instead, the "nut" has to be handled individually. Some of the nuts that wrote to Alvarez did align themselves with a movement like creationism, but they were often only sympathetic to the creationist movement, not central figures or even well informed participants. The only way to appreciate science nuts is to explore the Nutfile and see what they have to say. I asked physicist and Nobel laureate Steven Weinberg if he had a Nutfile. He did, but thought it would be disrespectful to share the letters with a historian. Our best bet is a famous physicist from the golden age of typed, non-digital letters who kept nearly everything, even letters from nuts.

EGYPTIAN PYRAMIDS: ALVAREZ'S INTRODUCTION TO THE NUTS

Two events first put Alvarez on the science outsider's radar. In 1968, Alvarez won the Nobel Prize in physics; a short time later, while still in Sweden, he announced

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[73] *ibid*., p. 12.

the first results of his pyramid work. Nobel laureates are all too aware of the attention it brings from non-experts with implausible theories and wild claims. However, Alvarez seemed to be deliberately stepping into the nuts' territory from 1964 to 1969 when he led an effort to find any undiscovered chambers in Chephren's Pyramid in Egypt. In a way, this search was quintessentially Alvarez—cosmic ray detectors, computerized particle counters, and an imaginative application of high-energy physics. However, Egyptian pyramids have long been a magnet for fantastic theories about aliens, conspiracies, and the like. Alvarez's scientific approach to finding hidden chambers was purely mainstream science in that he and his collaborators had the credentials, the funding, and the techniques of an uncontroversial, yet curious, experiment. When Alvarez crossed over from particle physics to Egyptology, we see what happens when the borders of conventional science are explored.

The Egyptian pyramids have long attracted nuts.[74] At least as far back as 1859, pyramidologists like the English popular science writer John Taylor theorized unconventional explanations for the pyramids. The Astronomer-Royal of Scotland, Charles Piazzi Smyth bolstered the field of pyramidology by expanding on Taylor's work. The 1970s saw a resurgence in pyramidology during that era of New Age mysticism with books like Patrick Flanagan's *Pyramid Power: The Millennium Science* in 1973, Max Toth and Greg Nielson's *Pyramid Power* in 1974, and Warren Smith's *The Secret Forces of Pyramids* in 1975. All kinds of mystical or religious theories abound

[74] Thanks to U.C. Santa Barbara graduate student Jason Linn for enlightening me on the history of "pyramidiots." Jason Linn, "Not Suffering Fools Gladly," unpublished manuscript.

about the origins and powers of the pyramids. Alvarez wrote of the "many letters from people whom archaeologists call 'pyramidiots.'"[75] But for Alvarez, the "pyramid project is more or less a hobby with me."[76] Biographer Richard Rhodes reported that "Oh, he did those things for fun"; it was a way "to make his physics work in very practical ways."[77] As Alvarez would find out, it was more than just a hobby for a lot of the nuts. Many of them took pyramids very seriously.

Probably 4,500 years ago, the Egyptian pharaoh Chephren, also known as Khafra, had a pyramid built as a tomb, as well as a human-lion statue known as the Sphinx. Chephren's pyramid was the second largest built at Giza—it would not be until 1884 that another man-made structure would surpass its height, and even then, the Washington Monument was only one percent of its bulk.[78] In his autobiography, Alvarez described the pyramids enthusiastically and with a curiosity about how they were built. He dismissed the theory that ancient Egyptians built a long ramp to build the pyramids—the ramp would have to have been much larger than the pyramids themselves! He further decided that Chephren's pyramid must have undiscovered chambers because the pyramids built by Chephren's father and grandfather had more chambers, and Chephren would surely have wanted to have at least as many places to hide his treasures as his predecessors.

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[75] Alvarez to Gershon Legman, editor of *Kryptadia: The Journal of Erotic Folklore*, 28 Jan. 1971, LWAP, box 32, folder "Miscellaneous 1971-72." Cited in Linn, "Not Suffering Fools Gladly."
[76] Alvarez to David A. Bundberg 31 Jan. 1967, LWAP, box 32, folder "Requests for Articles."
[77] Rhodes interview by author, 20 May 2009, p. 11.
[78] Alvarez, *Adventures*, p. 230. Other sources suggest other tallest structures before 1884, but only by a few years out of 4,500, so there seems to be no need to quibble.

The scanner used in the search for undiscovered chambers was relatively simple in principle, but required Alvarez's particular set of skills. The basic idea was to put a detector in the central Belzoni chamber and count how many cosmic rays penetrated from every angle. A chamber filled only with air would offer less resistance to the rays than would solid stone and so there would be slightly more particles from the direction of such a chamber. As discussed in chapter six, Alvarez's graduate work involved building directional Geiger counters to study cosmic rays at the University of Chicago and in Mexico City. Later, as his particle detectors became bigger and started producing mountains of data, Alvarez innovated the use of computers and artificial intelligence to automate data collecting. His Nobel work involved an IBM Mark II computer he named "Frankenstein."[79] Alvarez decided that, of all the particles showering down on the earth in cosmic rays, muons would have just the right penetrating power to make their way through an Egyptian pyramid, yet be partly filtered out by the mass of stone. Having just the right background, Alvarez designed a directional muon detector that was monitored by computer, a combination of his Geiger counter roots and his Nobel work on automatic data collecting and processing. Specifically, it was two parallel plates designed to momentarily shine at the point a muon crossed each plane—a scintillation in physics speak—and a camera and computer to count each pair of scintillations and calculate the angle from which the muon came. The computer was donated by IBM and was the first

[79] Alvarez, Nobel Lecture. "Recent Developments in Particle Physics," 11 December 1968. Reprinted in W. Peter Trower, ed., *Discovering Alvarez: Selected Works of Luis W. Alvarez with Commentary by His Students and Colleagues* (Chicago: University of Chicago Press, 1987), p. 129.

for an Egyptian university.[80] It was a simple but elegant solution suited exactly to this problem—Alvarez had to explain that "unfortunately, there aren't any other such objects to probe, as I tell the prospectors who frequently ask for my help in locating lodes of gold ore."[81]

Figure 5.1: Alvarez with Egyptologist Ahmed Fakhry and Pyramid Project team leader Jerry Anderson in Berkeley, 1967.

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80 Alvarez, *Adventures*, p. 233.
81 *ibid.*, p. 231.

The Alvarez team picked an interesting time to start this project. Alvarez was joined by the great Egyptologist Ahmed Fakhry and the head of the Ein Shams University physics department in Cairo, Fathy El Bedewi, creating a multinational team. The day after they started measuring, Israel and Egypt started the Six Day War. Some of the team members were seized and interrogated. Alvarez noted that Israel was able to defeat Egyptian radar and destroy the Egyptian air force on the ground. One team member was asked, "What were they *really* doing in Egypt."[82] Alvarez wondered to himself:

> I imagined myself an Egyptian intelligence officer trying to understand why Egyptian radar had failed to detect the incoming Israeli planes. In that role I remembered hearing about mysterious electronic equipment installed in the Second Pyramid, the finest of bomb shelters. The equipment was reported to have been put into operation on the day before the raid. I set to work to look into this fellow Alvarez. I discover that although he says he is doing archaeology, he has published no papers whatsoever in the field. Then I discover that he has patented an electronic system, VIXEN, that foxes radar systems so that their operators don't know an aircraft is approaching until it's less than a hundred yards away. I conclude that the diabolical Alvarez is a threat to my country and that the United States colluded with Israel to destroy the Egyptian air force.[83]

It would seem that Alvarez was very good at thinking like a conspiracy nut. As it turned out, the team was welcomed back with open arms. Alvarez was able to return in 1979 to celebrate the tenth anniversary of the Ein Shams University Computer Center that he had helped create. During that trip, Anwar Sadat and Menachem Begin embraced, signaling a major victory for peace in the Middle East.

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[82] *ibid.*, p. 234.
[83] *ibid.*

The pyramid work attracted the attention of a lot of nuts. What was already a hot topic for the nuts in Alvarez's Nutfile was fueled by the sometimes misleading press.[84] In 1969, the *Times of London* quoted one of the Egyptian technicians as saying, "either the geometry of the pyramid is in substantial error … or there is a mystery which is beyond explanation … which defies the laws of science."[85] Author Peter Tompkins picked up this quote in his 1971 *Secrets of the Great Pyramids*. In 1976, Alvarez had to continue dealing with the misquoted article. One set of data had been corrupted by a simple and common software bug that some interpreted as a supernatural phenomena described as "unexplainable, spooky data." He explained, "in order to make the story better, the newspaperman ignored the explanation … then the reporter supplied the gee-whiz statements that make the quote so memorable. The fact that this particular computer-derived error is common enough to have a well-recognized name—double binning—indicates that we weren't the first people to have experienced it."[86] It would seem that besides the standard nut, the press would often feed the sensationalistic need for a shocking story.

One essay in Alvarez's Nutfile by Howard J. Metz included a hand-drawn diagram of passages in an Egyptian pyramid.[87] After hearing about Alvarez's work in trying to find any secret chambers in Chephren's pyramid, Metz wanted to save him the trouble by telling Alvarez exactly where these chambers were. As it turned out, Alvarez

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[84] Thanks to Jason Linn for his work on Alvarez's difficult relationship to the press in his pyramid work. Linn, "Not Suffering Fools Gladly."

[85] John Tunstall, "Pyramid versus the Space Age," *London Times* 26 Jul. 1969. Cited in Linn, "Not Suffering Fools Gladly," p. 31.

[86] Alvarez to Richard Marten 28 Jun. 1976, LWAP, box 44, folder "Pyramid Project 1972-1985 [2 of 2]."

[87] Howard J. Metz essay, no title, no date, LWAP, box 38, folder 5.

managed to show that there were no chambers in Chephren's pyramid except for the long-known Belzoni Chamber in which Alvarez had placed his detectors. Apparently, Metz was wrong. Alvarez was often asked about failing to find a chamber in Chephren's pyramid, but he would reply: "We found that there wasn't any chamber."[88] It was important to him to show that his work was not a failure to find chambers; he framed it as a positive. Despite any desires to have found a major new discovery, Alvarez reacted like a scientist facing the reality of his results. Metz likely did not take well to being proved wrong.

DINOSAURS EXTINCTION WORK ATTRACTS NUTS

Already popular with nuts due to his wide-ranging career, Alvarez's dinosaur work made him an even bigger target. Dinosaurs are popular and well-known to the public; they are adored by children. Zoologist Stephen Jay Gould recounted a childhood trip to the American Museum of Natural History and seeing the *Tyrannosaurus* as the reason he became a scientist.[89] He also described the public furor when paleontologists decided that the *Brontosaurus* was an *Apatosaurus* and that the name *Brontosaurus*, popularized in television and movies, should be dropped.[90] Further, working on dinosaurs made Alvarez a target of creationists. A nut did not have to be well versed in creationism or the more recent Intelligent Design ideologies to criticize evolutionary

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[88] Alvarez, *Adventures*, p. 236.
[89] Gould, *The Panda's Thumb*, p. 267.
[90] Gould, "Bully for Brontosaurus" in *Bully For Brontosaurus: Reflections in Natural History* (New York: W.W. Norton & Company, 1991).

theory.[91] Many of the letters we have discussed try to promote some idea, often a "theory of everything," but Darwin deniers found it easier to tear down ideas. Finally, as we saw in the last chapter, dinosaur extinction is a case where Alvarez was an outsider to paleontology. Here, the difference between a scientifically legitimate outsider and a nut is rendered clearly.

In 1985, E.S.B. of Bensalem, Pennsylvania wrote to Alvarez about an article in June 1985 issue of *Popular Science* on the theory that a comet impact made the dinosaurs extinct: "If the scientific community believes that there is a great possibility of one of those comets hitting the earth in the near future, it would be adviseable [*sic*] for all of them to coordinate their efforts to stop the said comets or comet."[92] This is a perfectly rational reaction to a substantial misunderstanding of probabilities. If there really were a "great possibility" of another great impact, despite sixty-five million years of stability, then of course, immediate and decisive action would be necessary. However, this is not the place to argue with E.S.B. Instead, one should imagine Alvarez reading past the first few paragraphs to E.S.B.'s specifics: "key all telescopes outward to section L(45-1 1984561) telescope setting NW 80 degrees, upward deflection 80 degrees. Look for a star that is in the magnitude of one or more brightness. On a photograph it will appear as a star pattern that has never been seen before." Oddly, E.S.B. did not begin the letter with the warning that he had spotted a meteor—not comet—heading toward the

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[91] For a discussion of the history of creationism-related ideologies, see Ronald Numbers, *The Creationists: From Scientific Creationism to Intelligent Design* (Cambridge, Mass.: Harvard University Press, 2006).
[92] E.S.B., Bensalem, PA to Alvarez, 28 May 1985, LWAP, box 38, folder 4.

earth. Just as peculiar is E.S.B.'s choice in putting this data in the middle of the letter and moving on.

Often, even rational-sounding letters quickly descend into unmistakable nuttiness, despite their clearly sincere and hopeful nature. Perhaps one indicator of nuttiness missed by Bernstein and Baez is a lack of proportion. In a four page plan attached to E.S.B.'s letter, he elaborates on a system to store and reprocess water in tanks underground, and points out that the tanks must be "properly balanced so the weight of said containers will not alter the rotation of the earth."[93] Needless to say, even if this plan had to be enacted, the earth's rotation would be fine. Another common characteristic is the desire to connect current trends into theories or plans. E.S.B.'s "Lazer" stations benefit from the rise of Atari video games: "The people who will be using these lazer stations are the kids of today who are using the video games to gain practice with." Sometimes, idealism goes beyond wishful thinking about humanity's ability to work together, again in tune with the tendency to try to incorporate every trend. "There can be no room for prejudice for all people will be useful. … Each person or persons are already being guided to help in whatever way they can." However, this optimism quickly extends into abstraction: "All old visitation sites must be refurnished with laboritories [*sic*] and decompression chambers with altituedes [*sic*] and pressures to any system density and acclimated to take on any civilized being, no matter how strange they may look or be." Evidently, the new society's policy against racism is more intended towards aliens from outer space than from another human culture. E.S.B. may have included

[93] *ibid*.

himself in this outsider status, not for his race—whatever that was—or for how strange he might look or be, but for his ideas. The new society's culture accepted strange beings and strange ideas: "All ideas, no matter how impossible or illusive [*sic*] to complete must be used so earth can be protected." After all, the point of this letter was largely that E.S.B.'s ideas are as valuable as Alvarez's.

The tendency to incorporate widely disparate mysteries into one theory—the idea that the truth was looking at us right in the face: how could we miss it—is one of the most common patterns in nut letters. After Alvarez's meteor impact theory was described in a *Time* article in 1985, Nona, who we met earlier, wrote to Alvarez to comment on this theory: "Ahhh—can a theorist write to a physicist? I hope so."[94] Nona described her long-term interest in comet impacts, so much so that "they're even called 'the Nona theories'!" She continued, "You'd be annoyed—but here they are." Nona's theory was about "a 'huge' ice mass (1/3 the size of our present Earth)" that impacted the Pacific basin. As she pointed out, "ordinarily oil floats on H_2O. How did it get <u>under</u> our land masses." Furthermore, "A simple plane flight across the U.S. yields a most staggering vision of tossed up—cracked—mountains standing on edge. … How can we ignore this evidence or say the wind or rain did it?!" An explanation of geology seems unnecessary here, but perhaps it is worth noting that geologists' commitment to gradualism fed into this type of science misunderstanding. One who has read enough of

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[94] Nona, Bellevue, WA to Alvarez, 29 May 1985, LWAP, box 38, folder 4. Cited "Did comets kill the dinosaurs?" *Time* 125 (18): pp. 72-79.

these letters sees where this is going as the grammar increasingly reveals an urgency too emphatic for appropriately placed punctuation and capitalization:

> I know where the Aztecs + Mexican ancient folk went. they were literally swept away. by the catastrophe. Noah had time to build his ark—because the seas slowly rose. there are flood marks inside the Great Pyramid in Egypt. And this is so recent. It's within recorded history time. How can we not recognize it?

Nona was drawn to the same topics as other nuts: stories or civilizations that have captured the popular imagination in part because of their mystery. Spanish colonization destroyed much of the Aztecs' history and the pyramids in Egypt are so ancient that their story has been lost. Nuts often engage in speculation where professional scientists and historians cannot provide definitive answers. Baez's Crackpot Index could add points for introducing Noah's ark and for the insistence that the truth is imminently graspable to anybody who will open their eyes: "How can we not recognize it?"

Outsider scientists and opinionated laymen are generally defined by their rebel status as loners, but the creationists stand out from the writers of the other letters Alvarez relegated to the Nutfile for their unified beliefs. Instead, the creationists seem to share with the outsider scientists an intensity too angry for academic discourse. One handwritten, undated letter from "Knows better in Peoria, Ill." chastised Alvarez for his extinction theory and is worth quoting in full, with all the errors in the original:

> Your stupid theory about asteroids + Dinosaurs are as ridiculous as Charles Darwin on Evolution. He finally admitted <u>he was wrong</u> and I quote him "I was a young man with unformed ideas. I threw out queries, suggestions, wondering all the time over everything, and to my astonishment the ideas took like wildfire, People made a religion of them." Remarks such as these sound

> as if they came from a fool who's had his brains kicked out by a Missouri Jack ass.[95]

For our purposes, the significance of this quote is to tie "Knows better" to a broader movement by his use of the alleged Darwin quote, one widely used. This Charles Darwin quote is about an alleged discussion between him and a "Lady Hope," the British evangelist Elizabeth Reid, who published an account of Darwin's supposed change of heart in his last days. Her version of the story was published in the 19 August 1915 issue of the *Watchman-Examiner*, a Baptist journal in Boston. Discussions about the claim are widely available from both historians and creationists.[96] The story has been thoroughly debunked.

What is interesting about "Knows better" is that he distinguished himself among most other Nutfile writers—aside from the colorful language—by requiring a bit of digging. Apparently, to Alvarez, belonging to a larger movement does not qualify a person for escape from his Nutfile. Alvarez work on the impact theory of extinction supports the long age of the earth disparaged by creationists. So while the language, the "Jack ass" reference, and calling Alvarez's theory "stupid" may have been enough to easily categorize this letter, being from a creationist surely did not help. Based on a fairly thorough look at the letters in the Alvarez Papers, the correlation appears solid: creationists go in the Nutfile and never appear outside that box.

R.S.F. of the International Society of Analytical Trilogy, Inc. (ISAT), and of the Society of Integral Psychoanalysis (SIP), New York, NY wrote Alvarez a short letter

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[95] "Knows better in Peoria Ill" to Alvarez, no date, LWAP, box 38, folder 4.
[96] See, for example, James Moore, *The Darwin Legend* (Grand Rapids, Mich.: Baker Book House, 1994).

recommending a book by Marc Keppe.[97] An internet search shows that ISAT and SIP are still operating. According to their site, "Integral Psychoanalysis (Analytical Trilogy) is a scientific theory and methodology developed by psychoanalyst, philosopher and social scientist, Dr. Norberto R. Keppe, which unifies the fields of science, philosophy and spirituality."[98] Based in Brazil, the organization seeks to help people meet their full potential. R.S.F. added that Keppe explains that a great fire from sixty-five million years ago was caused by the ancient civilization of Atlantis using weapons with the power of our nuclear weapons. Although Alvarez would have been interested in understanding the nature of a cataclysmic event taking place sixty-five million years ago, he did not reply to this letter.

One series of letters from a "Prince Oan" dates from 1983 to 1986 and includes a twenty four-page letter dated May 5, 1983, or, more specifically, "Maius quintus, MCMLXXXIII." Labeled "SPECIAL" and addressed to "Walter, and/or Louis [*sic*.] Alvarez," it offers advice on the impact theory of mass extinction presented here without corrections:

> There is some merit to your asteroid theory if it were changed to asteroid Fragments, and/or, a Multitude of various sized asteroids that came down all over the Earth during a Flood that Destroyed descendants of first Adam, Dinos and all other large beasts, and Earth was moved closer to Sun so that every body of water was frozen and Earth was like a solid ball of Ice Eons of Time before Hebrew Adam was brought here from another world, The Earth moved back to it's normal orbit took a long time to thaw out and I believe ze Glaciers

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[97] R.S.F., New York, NY to Alvarez, no date (ca. 1985), LWAP, box 38, folder 4.
[98] http://www.trilogycongress.org/aboutus.html retrieved 15 Apr 2011.

> Pulverized ze surgace under them leaving a layer of clay, Heh! Heh! See D letter of Jan, 11, Ha-Makom, Baruk-Hu;[99]

Other letters by Prince Oan discuss Satan, the bible, political figures of the time, and reflect dissatisfaction with Carl Sagan, calling him "Carl Satan."

When Luis warned Walter and Muller that "they will laugh at you" for proposing an imperfect Nemesis theory, his careful approach contrasted with Officer and Drake's embarrassing presentation that resulted in literally being laughed at. The careful approach contrasts even more sharply with nuts who sometimes wore their outsider status like a badge of honor. Michael Shermer wrote that

> As paranormalists are fond of saying (after citing such notable blunders as Lord Kelvin's paper "proving" that heavier-than-air craft could not fly), "they laughed at the Wright Brothers." The standard rejoinder, made by skeptics for both levity and effect, is: "They also laughed at the Marx Brothers."[100]

The extreme examples in Alvarez's Nutfile, such as the letter from Prince Oan, are possibly not aware of the distinction, or at least we would have a difficult time understanding their thought process. Others, like Nona, are aware of the divide between themselves and Alvarez, but perhaps confuse the insider-outsider divide with the difference between a theorist and an experimentalist. Some may be confused by the demarcation issue and believe that their organization, for example, the International Society of Analytical Trilogy, has as much access to the truth as the Lawrence Berkeley Laboratory. Yet others, like "Knows better in Peoria, Ill.," reject the legitimacy of the establishment altogether. These are the obvious nuts. Their position on the legitimacy of

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[99] Prince Oan to Alvarez 5 May 1983, LWAP, box 38, folder 5.
[100] Michael Shermer, *The Borderlands of Science: Where Sense Meets Nonsense* (Oxford: Oxford University Press), p. 97.

insider science varies widely: either they want in, reject the boundaries, or are completely blind to them. For all the philosophers' work on the demarcation problem, we might wonder if John Baez had the right idea—crackpots tend to repeat the same mistakes over and over again. In J. C. Cooper we have a boundary case. His is not quite Shermer's "Borderlands science," but it managed to cross the border into a legitimate physics journal and required a physicist to respectfully refute his work in print.

J. C. Cooper and the Segrè Experiment

A controversy over faster-than-light particles made its way into Alvarez's Nutfile. J. C. Cooper distributed at least three undated essays to physicists some time in the late 1970s or early 1980s trying to convince the physics community that a famous 1955 experiment had accidentally proven the existence of faster-than-light particles. He became very critical of Emilio Segrè for his Nobel Prize-winning work, accusing him of a cover up. Cooper sent two of his essays to Alvarez, where they ended up in folder five of the Nutfile, with the other undated essays and clippings sent to Alvarez.

Why did he send the essays to Alvarez? Segrè and Alvarez had a long-running animosity, but it is difficult to say whether Cooper understood that or if he simply sent the essays to every prominent physicist he could think of. Segrè worked at Berkeley from 1938 to 1943 and again from 1946 to 1972, when he retired. He was at Los Alamos from 1943 to 1946, so between his time at Berkeley and Los Alamos, he spent most of his career working in close proximity to Alvarez. After all, many high-energy physicists at Berkeley had to cooperate with Alvarez because whatever their experiments, they needed someone to photograph and process the collisions and Alvarez's hydrogen bubble chambers were the best in the world.

Richard Rhodes said of Alvarez and Segrè, "who hated each other by the way," that "they each said that the other cheated on the work that gave them the Nobel Prize."[101] There was some controversy and a lot of animosity over credit for Segrè and Chamberlain's Nobel Prize. The experiment was conducted by Segrè, Chamberlain, Clyde Wiegand and Thomas Ypsilantis, but only Segrè and Chamberlain won the prize. Alvarez presided over a small conference taking place at Berkeley in 1985 celebrating the thirtieth anniversary of the antiproton experiment where Wiegand spoke for thirty minutes, barely mentioning Ypsilantis. Segrè reported that "He never mentioned me, as though I had not existed. I was saddened by the performance. It must represent Wiegand's present state of mind; this must be his recollection of the discovery of the antiproton."[102] At the same conference, physicist Oreste Piccioni spoke for about an hour, as Segrè recalled: "He renewed his accusations against Chamberlain and me, saying that we had stolen the plans of the apparatus used from him, and that I had by trickery excluded him from the execution of the experiment."[103] Piccioni had brought these accusations to lab chief Ernest Lawrence in a scathing letter in 1955 just as the experiment concluded. Segrè, who described Piccioni as a "brilliant physicist,"[104] replied that "Piccioni had made some good suggestions during the planning of the experiment,

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101 Rhodes interview by author, 20 May 2009, p. 22.

102 Emilio Segrè, *A Mind Always in Motion: the Autobiography of Emilio Segrè* (Berkeley: University of California Press, 1993), p. 316n9.

103 *ibid*.

104 *ibid*, p. 248.

and these were duly and repeatedly acknowledged in publication."[105] Lawrence concluded there was no misdeed. Piccioni sued Segrè and Chamberlain in 1972, but courts "from the Alameda superior court to the Supreme Court"[106] dismissed the case because the statute of limitations had passed. Segrè later reported that "a famous physicist" told him that "Oreste is crazy and you are not!"[107]

For all this tension, it is not clear that Alvarez ever took Piccioni's side or intervened at all. Segrè described Alvarez's working relationship with Lawrence's successor Edwin McMillan as an "implacable personal hostility."[108] Alvarez conceded that mounting "difficulties with Ed McMillan" were one of the reasons he took some time off from Berkeley to join the President's Science Advisory Committee in 1960.[109] Italian physicist Count Lorenzo Emo Capodilista warned Segrè about Alvarez, describing him as "a little fascist leader, fawning to the Duce, but mean to his equals or inferiors."[110] Presumably, "the Duce" was Ernest Lawrence, since Alvarez was famously loyal to the head of Berkeley's Radiation Laboratory. In contrast, Segrè never got along with Lawrence. Segrè even recalled an incident where Alvarez barged into his office: "Luis Alvarez attacked me."[111] This was 1950, around the time that the Regents of the University of California required a loyalty oath of its faculty in the buildup to the Cold War. Several faculty refused and were fired, although the oath was later ruled

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105 *ibid*, p. 257.
106 *ibid*.
107 *ibid*, p. 258.
108 *ibid*, p. 267.
109 Alvarez, *Adventures*, p. 218.
110 Segrè, *A Mind Always in Motion*, p. 135.
111 *ibid*, p. 237.

unconstitutional. Segrè circulated a pronouncement by Pope Pius XI saying that a person could disingenuously sign a loyalty oath with no moral reservations in cases such as the rising fascism in Italy.

A physicist and associate of Segrè's, Bruno Pontecorvo, defected to the Soviet Union unexpectedly, making Segrè's stance on the loyalty oath suspect in the eyes of those who pushed for an oath. Alvarez accused Segrè of disloyalty over the value of a patent, as recorded in Segrè's notes:

> Alvarez enters my office. O. Chamberlain present. Asks about Pontecorvo. Then says it is improper to ask for compensation for the Fermi patent because we came to this country and the shelter received was to us worth more than a million dollars. We are guests here and we should be glad to be able to repay in part the USA for the privilege of citizenship. I answer that I did not think that citizenships were for sale. That the law fixed such things. He said that to bring a suit (for patent compensation) was like settling a quarrel by fisticuffs in a bar. I answered that US court will not be flattered by the comparison. He concluded that I should let him know when Pontecorvo writes me from Russia.[112]

To be fair to Alvarez, he was consistent on patent ownership for inventions discovered while working for the military: his basic patent for radar transponders, now standard equipment on all commercial aircraft, earned him "the standard 'one dollar plus other [nonexistent] valuable considerations.'"[113] However, there is no question that Alvarez could be abrasive and that he and Segrè did not get along. Richard Rhodes once asked Segrè "why he didn't want to write about it. He said, 'That's for after I'm dead,'

[112] *ibid*.
[113] Alvarez, *Adventures*, p. 218.

meaning basically, 'I don't want to rock the boat.'"[114] His wife, Rosa, noted that as the reason that Emilio wanted his autobiography published posthumously.[115]

Alvarez disagreed that Segrè and he had such hatred, but perhaps such heated interactions seemed normal to him. He described his relationship with Segrè in a draft of his autobiography in a paragraph that was cut from the book:

> Emilio and I have always been mysteries to each other, since we work so differently and we do physics in such a different manner. But we obviously have great mutual respect; we enjoy talking physics together, since each of us brings something to a conversation that the other might not otherwise have known. We have never had a close personal relationship, but we so often eat lunch together that most physicists at the laboratory probably feel that in spite of some rivalry we've had in the past, we are nonetheless good friends.[116]

Alvarez and Segrè sometimes begin anecdotes with the two of them talking over lunch, confirming that they could at least be cordial, even friendly. The question remains as to whether Cooper was aware of the tension and whether he hoped to capitalize on it.

In 1979, Cooper published a critique of the paper that earned Emilio Segrè and Owen Chamberlain the 1959 Nobel Prize in physics for the discovery of the antiproton.[117] In 1955, Segrè and Chamberlain, along with Wiegand and Ypsilantis, identified antiprotons in a stream of pions generated in the Bevatron at Lawrence Berkeley Laboratories.[118] Cooper's problem was not with the antiprotons, but with the data that was thrown out. He calculated from Segrè and Chamberlain's published data

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[114] Rhodes interview by author, 20 May 2009, p. 23.
[115] Rosa Segrè, "A Few Words from Rosa," in Segrè, *A Mind Always in Motion*, p. 297.
[116] Alvarez autobiography draft, LWAP, box 46, folder "Chapter XVII Autobiography pp 541-582 1972," pp. 555-6.
[117] J. C. Cooper, "Have Faster-Than-Light Particles Already Been Detected?" *Foundations of Physics* 9, nos. 5/6 (Jun. 1979), p. 461.
[118] Owen Chamberlain, Emilio Segrè, Clyde Wiegand, and Thomas Ypsilantis, "Observations of Antiprotons," *Physical Review* 100, no. 3 (Nov. 1955), p. 947.

that the pions were travelling at an average velocity of 1.0525870*c*, or 5.2587% faster than the speed of light, a violation of Einstein's theory of relativity. Further, he calculated a margin of error implying a probability of 99.72% that Segrè and Chamberlain had in fact discovered faster-than-light particles: tachyons.

Anyone familiar with high school laboratory work may be suspicious of the eight-digit accuracy of the 1.0525870 figure. Furthermore, tachyons are not mainstream science, instead taking their most prominent place in science fiction such as "Star Trek." They are theoretical particles not in the sense that some theory predicts them, but in the hypothetical sense that if faster-than-light particles exists, we should call them tachyons. The term was coined by physicists Olexa-Myron Bilaniuk and E. C. George Sudarshan in 1969.[119] Einstein's special theory of relativity establishes that nothing can go faster than light, but closer inspection shows that travelling at the speed of light is the problem, not actually going faster. In principle, a particle could go faster than light if it never crossed the light-speed barrier and slowed to below the speed of light. Such particles have not been detected.[120]

However, our purpose here is not to debate Cooper's assertions. Fortunately, University of Colorado at Boulder physicist Allan Franklin has done the fact checking and concluded that Cooper's claims are ungrounded. Fortunate, that is, for the sake of closure. If Cooper had been right, he claims that science would yield limitless

[119] Olexa-Myron Bilaniuk and E. C. George Sudarshan, "Particles beyond the light barrier," *Physics Today* 22, no. 5 (May 1969), p. 43.

[120] Very recently, physicists at CERN claim to have detected a faster-than-light particle, but these are very preliminary results and have not been widely verified. Dennis Overbye, "Particles Faster Than the Speed of Light? Not So Fast, Some Say," *New York Times*, 24 Sep. 2011.

technological benefits. At the heart of his argument is that Segrè and Chamberlain—in fact the entire physics community—were committing an enormous cover up in order to protect the status quo.

The 1955 paper in question is "Observation of Antiprotons."[121] Paul Dirac's work on anti-electrons, or positrons, implied the existence of an antiparticle for the proton. The Lawrence Berkeley Laboratory developed a particle accelerator called the Bevatron, named for its ability to generate beams with energies in the billions of electron volts (B.e.v.) in 1954—powerful enough to create antiprotons. The following year, Segrè and Chamberlain succeeded in observing antiprotons by aiming a beam of protons at a copper target and diverting the resulting particles into a system of magnets and counters designed to identify antiprotons. After one magnet redirected the particles and another magnet focused the beam, a heavy shield with a small hole allowed through only particles with a specific momentum. By using a particle counter of a special design—a Cerenkov counter—Segrè and Chamberlain were able to allow through only particles with a very specific velocity, between $0.75c$ and $0.78c$. As an alternate test of the particles' velocity, the distance between two counters was known, as was the time of transit between them. The vast majority of particles generated by the Bevatron were pions, which are composed of two quarks, while protons and antiprotons are both composed of three quarks. Given the extra mass, Segrè and Chamberlain were able to distinguish the antiprotons from the pions: pions would be travelling faster given an equal momentum. The negative charge on an antiproton would make sure that only antiprotons

[121] Chamberlain, "Observations."

made it through the first magnet. As a final test, they reversed the fields on the diverting magnets to ensure they could identify regular protons. When time came for the final experiment, their counters detected about one antiproton for every 44,000 pions. Segrè and Chamberlain had succeeded in confirming the existence of antiprotons and would be awarded the Nobel Prize in Physics for their work in 1959.

Cooper did not deny the existence of antiprotons. He instead looked at the pions traveling at very near the speed of light. To create enough energy to create antiprotons, the Bevatron had to accelerate the protons to a speed that would create extremely fast pions traveling at very near the speed of light. Segrè and Chamberlain believed that speed to be $0.99c$ based on the known mass of a pion and the momentum given to them by the Bevatron, as well as the principles of special relativity. Cooper instead used the published figures of pions traveling 40 feet in 37 to 41 nanoseconds to calculate a velocity slightly higher than the speed of light. Segrè and Chamberlain were not interested in the speed of the pions.

Cooper said he accidentally discovered this violation of the theory of relativity in 1976 and tried to report his results to academia. After some frustration, he tried publicizing his results in 1978 "to many in the news media business."[122] After the news media told him they did not have the expertise to judge his claims, he finally succeeded in getting noticed by getting "Have Faster-Than-Light Particles Already Been Detected?" published in *Foundations of Physics* in 1979. In this six-page article, Cooper statistically

[122] Cooper, "A Marvellous Work and a Wonder: For the Wisdom of Their Wise Men Shall Perish," undated essay, LWAP, box 38, folder 5, p. 2.

analyzed the pion flight times as reported in the Segrè article and tried to cover all possible explanations for why the pions might have been going slower than the speed of light. He concludes that Segrè and Chamberlain would have to have misreported the forty-foot flight path by at least two feet, which Cooper decided would be implausible. Further, if the flight path were wrong, then the velocity of the antiprotons must have been wrong as well, meaning that the reported mass of the antiproton was wrong. This sort of sloppy measurement would, in Cooper's mind, lead to all sorts of trouble for the physics community and must be rectified. Finally, Cooper suggested that the discovery of faster-than-light particles would be a boon to physics and would help explain some phenomena in astrophysics. In his concluding paragraph, he cited five recent astronomy articles that would benefit from the discovery of faster-than-light particles.

The December 1982 issue of *Foundations of Physics* included University of Colorado at Boulder physicist Allan Franklin's critique of Cooper's 1979 article and Cooper's rebuttal.[123] Franklin argued that Cooper had fixated on the forty-foot distance between two counters. Segrè and Chamberlain had used the speed-sensitive Cerenkov counters and the known momentum to estimate the antiproton mass and any measurement of the pions was incidental. The reported forty feet was an estimate used to perform a reasonable confirmation of the primary data. Franklin's article was a succinct, but biting two pages: "It is not the purpose of this paper to comment on Cooper's statistical analysis

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[123] Allan Franklin, "Cooper's Evidence for Faster-Than-Light Particles," *Foundations of Physics* 12, no. 12 (Dec. 1982), p. 1181 and Cooper, "Faster-Than-Light Particles: A Reply to Franklin," *Foundations of Physics* 12, no 12 (Dec. 1982), p. 1183.

and estimates of error, which deserve criticism, but rather to again analyze the data to appraise Cooper's conclusion."[124] Franklin cited only the Cooper paper and the original Segrè paper, but did refer to private communication with Emilio Segrè to back up his claim that "Although Cooper makes much of the published counter distance of 40 feet, it is clear from comments from the experimenters that this was only a crude measurement of the distance, and as we shall see below, a reasonably accurate one."[125] He concluded that "there is no evidence to support Cooper's claim that faster-than-light particles were observed in this experiment."[126]

Cooper responded with a ten page article starting with a seven page rebuttal of Franklin's argument. While he had previously omitted a discussion of the speed sensitive Cerenkov counters, he now addressed this hole in his argument. He replied that the "Velocity selector, C2, was merely a Cerenkov counter detecting particles traveling in a narrow range from $0.78c$ to $0.75c$."[127] It is not clear what is "merely" about the Cerenkov counters, but he referred to them this way twice. He countered Franklin's claim that he was "fixated" on the forty-foot measurement with the claim that "When Franklin states the antiproton mass is determined independently of the S1-to-S2 distance, he is clearly wrong,"[128] suggesting that Franklin is equally fixated with the Cerenkov counters. He promptly returned to a discussion of that forty-foot distance. Admittedly, Cooper's

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124 Franklin, "Cooper's," p. 1181.
125 *ibid*., pp. 1181-2.
126 *ibid*., p. 1182.
127 Cooper, "Reply to Franklin," p. 1186. In this article, he began referring to faster-than-light particles as tachyons.
128 *ibid*., p. 1188.

arguments were strengthened by Segrè and Chamberlain's inconsistent reporting on the precision of the forty-foot measurement: telling Franklin it was a "crude measurement,"[129] telling *Scientific American* that it was "precisely 40 feet"[130] and reporting in Chamberlain's Nobel Acceptance speech that it was "12 meters" (39.37 feet).[131] However, it must be stressed that Segrè and Chamberlain were not interested in the speed of the pions, since they were looking for the slower antiprotons. Some fudging on tangential data is forgivable.

The confusion on Cooper's part is understandable. In an interview conducted in 1967, historians Charles Weiner and Barry Richman asked Alvarez about his work on time-of-flight methods that were initially used to isolate slow neutrons for the Manhattan Project. Alvarez replied that "I really don't feel that I had a terrible lot to do with that,"[132] but Weiner and Richman asked him more on the use of time-of-flight measurements:

> Weiner: When did it [time-of-flight measurements] start to be used in this widespread way?
>
> Alvarez: Shortly after the war.
>
> Weiner: Then it became widely known.
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> Alvarez: Yes, it became the standard technique.
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> Richman: It was used in the anti-proton discovery?

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[129] Franklin, "Cooper's," cited in Cooper, "Reply to Franklin," p. 1183.
[130] Cooper, "Reply to Franklin," p. 40. He cites Segrè and Wiegand, *Scientific American*, vol. 194, no. 37 (Jun. 1956).
[131] Owen Chamberlain, "Nobel Lecture," 11 Dec 1959, in Bengt Samuelson and Michael Sohlman, ed., *Physics 1942-1962. Nobel Lectures, including presentation speeches and laureates' biographies*, (Singapore: World Scientific, 1998), p. 496, cited in Cooper, "Reply to Franklin."
[132] Alvarez interview by Charles Weiner and Barry Richman 15 Feb. 1967, session II, p. 10.

> Alvarez: Well, people have been using time-of-flight in everything now because we have such fast circuits and we have counters that measure to a fraction of a nanosecond; that's a time that high-speed particles [take to] go a few inches.[133]

Presumably, the counters Alvarez was talking about were the Cerenkov counters that Segrè and Chamberlain used. But the interesting part of this exchange is that Richman thinks the time-of-flight method was used by Segrè and Chamberlain, while Franklin pointed out that time-of-flight was only a way to double check the data, not the main technique. Alvarez did not correct Richman, instead giving a vague answer about the ubiquity of the method in 1967 when the interview happened, not 1952 when the experiment happened. It seems unlikely that Alvarez meant anything by leaving that vague, but again, it makes that misunderstanding seem more plausible.

However, Cooper went beyond the statistical analysis in this paper. After seven pages of tedious but focused discussion of the forty-foot distance between detectors, Cooper began to show the usual signs of a candidate for the Nutfile. After calculating a relatively conservative 87.43% chance that Segrè and Chamberlain had discovered tachyons, Cooper noted that this is roughly seven out of eight: "One can imagine a revolver with 8 bullet chambers, of which 7 are loaded with bullets and one is empty. After spinning the cylinder, no rational man would hold the gun to his head to play Russian roulette."[134] While this might be a fair analogy, it does seem striking that, to Cooper, this is analogous to a life-and-death affair, while to Segrè, it was just a way to find antiprotons. Cooper then produced a 99.72% chance of tachyons based on less

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[133] *ibid*.
[134] Cooper, "Reply to Franklin," p. 1189.

conservative assumptions and noted the 356-to-1 odds: "One can imagine a revolver with 357 bullet chambers, of which 356 are filled with bullets and one is empty. Surely no rational man would play Russian roulette with these odds, either."[135] Whatever the odds, Cooper went on to describe some of the potential benefits of tachyons, citing eight papers that discuss tachyons as proof that these theoretical particles do have some respectability in the physics community.

The most interesting article, from Cooper's perspective, is a *Physical Review* article by F. A. E. Pirani[136] disproving the existence of tachyons. Pirani provided a simple proof that the existence of faster-than-light particles would violate the principle of causation, that is, that tachyons could reverse the normal cause and effect relationship: effects could precede the cause. Pirani provided a simple thought experiment showing how it could be done. Cooper noted that "Pirani is credited with the most rigorous proof of this proposition,"[137] but turned the proof upside-down by extending Pirani's thought experiment into a workable machine for sending messages back in time. That is, Piriani was arguing that tachyons cannot exist because they would violate the important principle of cause and effect. Footnote seventeen in Cooper's paper, unlike the rest of his footnotes, is not a reference to a refereed physics journal or published book: it is a patent application for such a machine by a J. C. Cooper.[138]

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[135] *ibid*., pp. 1189-90.
[136] F.A.E. Pirani, "Noncausal Behavior of Classical Tachyons," *Physical Review D* 1, no. 4 (15 Jun. 1970), p. 1.
[137] *ibid*., p. 1190.
[138] *ibid*.

Cooper cited four papers published in refereed journals that question this "*a priori* assumption that causes must chronologically precede their own effects,"[139] praising one author for being "bold enough to title his paper, 'Can the Future Influence the Present?'"[140] However, the author of that paper, R. D. Driver, noted in that paper that "The question posed in the title of this paper may be impossible to answer since it may eventually reduce to a matter of interpretation."[141] He continued: "A more precise title for this paper would have been: 'An existence and uniqueness theorem for the classical relativistic model of two electrons in one-dimensional motion with half-retarded-half-advanced interactions.'"[142] Cooper might have expected Driver to hedge his bets. As it turned out, Cooper had a very low estimation of the creativity of professional physicists.

After suggesting that physicists generally reject radical new ideas like his, Cooper argued:

> I am disturbed by this attitude that dominates segments of the physics community. If an experiment's results are consistent with preconceived ideas, those results are accepted. But, if an experiment surprises physicists with data that conflict with old laws and preconceived ideas or if the data suggest phenomena physicists do not understand, the data are dismissed and rationalized as being caused by unknown and unproven systematic errors or simply a statistical fluke. Thus, few really significant new discoveries can ever be made because the utility of the experiment in scientific investigation has been destroyed; this attitude condemns the physics community to stagnation.[143]

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[139] *ibid*.
[140] *ibid*.
[141] R. D. Driver, "Can the Future Influence the Present?," *Physical Review D* 19, no. 4 (15 Feb. 1979), p. 1106.
[142] *ibid*., p. 1107.
[143] Cooper, "Reply to Franklin," p. 1191.

Cooper seems to be echoing a cynical version of Thomas Kuhn's paradigms. Kuhn argued that scientists establish a body of knowledge that ignores anomalous data that cannot be made to fit in the existing paradigm and that it often takes a new generation of scientists to break with the old model. However, this was not a criticism of science, just a statement about how Kuhn saw progress at work in science. Kuhn realized that science critics and outsiders used his idea of paradigm shifts to argue for the relativism of science—we can disregard any theory in science because a new one will eventually replace it. This was not his intent, as he explained in a postscript to a later edition of his *The Structure of Scientific Revolutions*.[144] We cannot be sure that Cooper read Kuhn's work originally published in 1962, but the idea of paradigm shifts had become ubiquitous even outside the history of science. Cooper saw people ignoring his work on tachyons and assumed that he was that young physicist with anomalous data who would lead a new scientific revolution. While Cooper did not explicitly compare himself to Galileo, Newton, or Einstein, Baez's Crackpot Index comes to mind.

An *American Journal of Physics* study from 1980 seems to confirm Cooper's suspicions that the period from 1940 on was a stagnant period in physics.[145] Further, George Gamow's 1966 book *Thirty Years That Shook Physics* corroborates the idea that innovation in physics had come to an end by 1940.[146] After all, Gamow was not arguing that physics after 1940 had continued to "shake." Cooper suggested that this was a fairly

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[144] Kuhn, *The Structure of Scientific Revolutions*, pp. 205-7.
[145] C. Sharp Cook, "Is Physics Approaching a State of Stagnation?" *American Journal of Physics* 48, no. 3 (Mar. 1980), p. 175.
[146] George Gamow, *Thirty Years That Shook Physics*, (Double Day: Garden City, N.Y., 1966), p. 161, cited in Cooper, "Reply to Franklin," p. 1191.

recent development: he argued that physicists "refused to dismiss the Michelson-Morley results with speculation and rationalization that the results might have been caused by unknown systematic errors. Instead, they repeated the experiment and, to their surprise, confirmed the startling data which ushered in relativity."[147] This was exactly what Cooper requested, that the physics community repeat the original Segrè experiment and more carefully measure the velocity of the pions. It would seem that Cooper was making a fundamentally fair and rational request. It was a fundamentally sound strategy: restrict the strangest parts of a theory to personal correspondence.

Cooper wrote at least three unpublished essays that he distributed to physicists. Allan Franklin cited "More on the Segrè Experiment: or the Decline of Physics,"[148] which appears in the Luis Alvarez papers, and "Fraudulent Experiment Won 1959 Nobel Prize," which does not. The Alvarez papers also include "A Marvellous [*sic*] Work and a Wonder: For the Wisdom of Their Wise Men Shall Perish."[149] Franklin told me that he was not aware of the last paper, and if he had been, he would have ignored it.[150] He explained that a colleague asked him to look at the Cooper paper, and then the editor of *Foundations of Science* asked him if he wanted to reply to Cooper's reply. He declined.

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[147] Cooper, "Reply to Franklin," p. 1191. While Cooper got the history wrong here—repeated experiments by Dayton Miller and others did find some effect—he might be forgiven since it is the standard story told in physics textbooks.
[148] Allan Franklin, *The Neglect of Experiment* (Cambridge: Cambridge University Press, 1986), p. 279, note 34. The essay is in LWAP box 38, folder "Nutfile [5 of 5]."
[149] Cooper, "A Marvellous Work and a Wonder: For the Wisdom of Their Wise Men Shall Perish," no date, LWAP, box 38, folder 5.
[150] Personal communication, 16 Dec 2008.

Based on context, the eight-page "More on the Segrè Experiment" appears to have been written between July 1981 and May 1982, and largely reiterates the argument in "A Reply to Franklin," adding to the urgent tone. Apparently unfettered by journal referees, Cooper opened up a bit more. In keeping with the Nutfile label, Cooper alleges public opinion has already turned on Segrè and discusses the ramifications of his experiment in the long term: "The media's scathing ridicule is nothing compared to what historians will do to Segre and the current physics community over the *next several million years*."[151] He followed that with a quote from the book of Revelations and would repeatedly warn of the events of the "next several million years." His claim became more specific: Segrè had been "forced to lie and cover up" because he "could not find what was causing his mesons to travel faster than light."[152] Cooper thought that the cause of stagnation in physics was resistance against data that did not fit the accepted theoretical model. Perhaps he had been reading Thomas Kuhn's *The Structure of Scientific Revolutions*, but he did not mention it. It would seem that Kuhn's "paradigm shifts" had permeated its way through society. Besides Segrè, Cooper argued that textbook writers had joined the conspiracy. After a search through physics textbooks, Cooper found that they all omitted "Segrè's now infamous Figure 3 and the raw faster-than-light meson data."[153] He continued, describing himself in the third person: "Historians will have some very harsh comments about the physics community and will conclude that Cooper was not among even the first 1000 men to discover that Segre's mesons were travelling

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[151] Cooper, "More on Segre," p. 1. Emphasis added.
[152] *ibid*., p. 3.
[153] *ibid*., p. 5.

faster than light."[154] One way of undoing his outsider status was to knock down the walls: "Physics has declined to a pseudo-science."[155] Cooper seems to believe he solved the demarcation problem by discrediting the entire field of physics.

What of Cooper's claim that the media had been ridiculing Segrè?[156] He noted three articles in *Omni* and one in *Science Digest* suggesting that physicists sometimes cheated and often defended existing theories. However, the bulk of his argument was that fiction writers were mocking Segrè. One only had to look to a handful of serialized novels in *Omni* and *Analog* magazines and to an episode on the "Lou Grant" television show, each featuring story lines about deception in physics. Apparently, these fictional story lines were meant to illustrate a real problem in physics. Cooper could find evidence everywhere he looked, despite questionable sources. He closed with a plea to redo the experiment. Arguably, his was a good strategy: keep the more extreme claims in personal correspondence and the physics in the journals. However, his next essay suggests he may have been better off keeping all of it to himself.

What, then, is Cooper's larger point? Why is he so obsessed with a singular example of what he believes to be a fraud? His unpublished essay, "A Marvellous Work and a Wonder: For the Wisdom of Their Wise Men Shall Perish" explains the context. As it turns out, there is a bigger story here behind the Segrè experiment, the cover-up, Cooper's warnings, and the physics community's resistance to tachyons and time machines, and it comes out of a biblical prophesy:

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[154] *ibid*.
[155] *ibid*.
[156] *ibid*., pp. 7-8.

> According to this 3400-year-old prophecy, the world will initially disbelieve a man who will report 'a marvellous work and a wonder: for the wisdom of their wise men shall perish.' this man will report a discovery of an 'unheard-of natural phenomenon' first to the 'learned' who reject this truth because of their prejudice, then to the 'not learned' who reject his truth because of their misplaced trust in the 'learned.' Despite the great evidence before them, the wise men will 'meet with darkness in the daytime, and grope in the noonday as in the night,' and 'they are drunken, but not with wine; they stagger, but not with strong drink.' This great event will occur during a dark period when wise men will find it difficult to discover truth and when God 'disappointeth the devices of the crafty' who 'cannot perform their enterprise' of discovering truth. According to the prophecy, several wise men will be exposed as 'liars' as 'He catches the wise in their own ruses.'[157]

The title of the essay, the quote about the perishing wisdom of wise men, and the quote about strong drink are from Isaiah 29:14, King James version. Most of the remaining quotes are from Job 5, except for the final quote, which is from 1 Corinthians 3:19. Theology is a serious field, but citing scripture to back up an argument about particle physics erases any doubt that Cooper belonged in the Nutfile. The idea that old wisdom was overthrown with new revelations is a theme in both the Old and New Testaments, but that is no reason to believe that Cooper derived legitimacy from those prophets.

According to Cooper, his work was one part of that prophecy. "My purpose is not to convince you. My purpose is merely to make an historical record that I declared the message unto you."[158] He added that stubbornness on the part of the physics community was the other part of the prophecy. "Had Segre announced that his mesons were travelling [*sic*] faster than light, not only would he not have received his Nobel Prize for his discovery of the antiproton, but the scientific community would have crucified

[157] Cooper, "Marvellous Work," p. 1.
[158] *ibid*., p. 3. It is not clear if "you" refers specifically to Alvarez, but it probably refers to anyone reading his essay.

him."[159] Besides his well-worn story about the forty-foot flight path, Cooper had scriptural support: "The prophesy in Isaiah 44:25 does not quibble. God describes Segre et al as 'liars.'"[160] Needless to say, Isaiah 44:25 does not explicitly mention Segrè, only the idea that God supersedes human wisdom. The seriousness of the situation was clear to Cooper, who described Director of the Lawrence Berkeley Laboratory Andrew "Sessler and Segre et al as the archvillians [*sic*] of humanity."[161]

It is sometimes noted that scripture can be used to prove anything, especially by a person like Cooper, who was willing to be flexible with biblical interpretation. After all, Cooper argued that "One can have complete confidence in biblical prophecies which predict the downfall and humiliation of the physics community because biblical prophesies have never failed to come true."[162] However, Cooper was good at combining a series of events and scattered and otherwise unrelated scripture into a grand narrative. In the penultimate section of his final essay, Cooper would finally reveal his message.

Cooper's ultimate goal was to reveal the true nature of God: "God originated, not from another part of the universe and not from our past, but from our own civilization at a time yet future."[163] For Cooper, modern medicine was getting us closer to immortality; cybernetics and information technology approached omniscience; test-tube baby technology could impregnate a virgin; all that was missing was a time machine made

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159 *ibid*., p. 11.
160 *ibid*., p. 13.
161 *ibid*., p. 14.
162 *ibid*., p. 18.
163 *ibid*.

possible with tachyons. For an ancient civilization, modern technology might seem miraculous. Furthermore, Cooper found a great many biblical passages that (loosely) supported his claims. He thought that "the Jews threw stones at Christ because the Jews knew nothing about time travel or Feynman diagrams where nuclear particles mathematically travel backward in time and have an existence prior to their creation."[164] Can any true "nut" leave out the Jews?

To be fair to Cooper, physicist Richard Feynman did advocate an interpretation of quantum mechanics, his Quantum Electrodynamics (QED), that interpreted some particles as ones travelling back in time: "The backwards-moving electron when viewed with time moving forwards appears the same as an ordinary electron, except it's attracted to normal electrons—we say it has a 'positive charge.' … For this reason, it's called a 'positron.' The positron is a sister particle to the electron, and is an example of an 'anti-particle.'"[165] QED is on extremely solid ground experimentally and antiparticles do behave like regular particles with negative time, but nobody in the physics mainstream believes that could lead to us travelling backwards in time.

J. C. Cooper entered this story seeming like the most rational of the Nutfile correspondents, but he exited possibly more "nutty" than any of the others. He had published two articles in a physics journal based on a superficial, but plausible argument. However, the close reader almost certainly noted that he did not give his

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[164] *ibid*., p. 21.

[165] Richard Feynman, *QED: the Strange Theory of Light and Matter* (Princeton: Princeton University Press, 1985), p. 98. To clarify, if the wave equation for an electron has occurrences of t (time) replaced with $-t$, the resulting particle behaves like a positron, identical to an electron but with a positive charge.

academic affiliation or background. Instead of a university address, *Foundations of Physics* gave only his hometown: Indianapolis, Indiana in the first article, then Riverside, California in the second. He made a great deal of the fact that he had published twice in this journal: "Foundations of Physics is a prestigious international physics journal whose 27-member board of editors includes Nobel laureates, such as, Louis de Broglie, Murray Gell-Mann, Ilya Prigogine, Albert Szent-Gyorgyi, Eugene P. Wigner, and Chen-Ning Yang, and other prominent scientists, such as, Sir Karl Popper, Henry Margenau of Yale, and Robert H. Dicke of Princeton."[166] He was, of course, borrowing language from *Foundations of Physics*. Yet he did not attempt to hide his lack of training. He was a lawyer with little or no training in physics or theology: "I do not have a doctorate in physics, or a masters degree, or an undergraduate physics major, or even an undergraduate physics minor for that matter."[167] He argued that his outsider status helped him, since he was not subject to the forces that kept professional physicists like Segrè from telling the truth. "Only a maverick, an outsider, a ne'er-do-well like me can afford to risk the wrath and condemnation of the scientific community."[168] Of course, he provided some scripture to prove that an outsider would overturn the work of the learned.

Cooper claimed to be a proponent of the scientific method and was confident that he was doing mainstream science. If professional physicists could not see that, then they were not really scientists; he called modern physics a "pseudo-science." We do not know if he read Thomas Kuhn, but his is a common misinterpretation of the "normal science"

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[166] Cooper, "Marvellous Work," pp. 4-5.
[167] *ibid*., p. 21.
[168] *ibid*.

that comes between scientific revolutions. Science critics often assume that normal science is a flawed because a revolution will inevitably come and wipe away all the flawed assumptions that scientists will fruitlessly defend. In fact, normal science is a necessary mode of doing science. How can a scientist build on the work of others if he does not assume that the previous work is generally valid?

Would Bernstein and Baez's tests have identified Cooper as a crank? Obviously, "A Marvellous Work and a Wonder" exposed Cooper as a classic nut, with biblical arguments, overreaching theories, conspiracies, and grave consequences for those who ignore him. Baez's Crackpot Index would not have detected anything out of the norm in Cooper's published papers, although if Baez read Cooper's paper, he might add two items to his list: "does not understand significant digits" and "focuses intently on tangential details." But would Bernstein have spotted a nut if he had been working at *Foundations of Physics* and received Cooper's "Have Faster-Than-Light Particles Already Been Detected?"? Did Cooper's published paper pass the tests of correspondence and predictiveness? Cooper's claim that Segrè created tachyons is largely compatible with mainstream physics. Tachyons would simply be particles that have not yet been discovered. They could never cross the light barrier, but as long as they continued to travel at faster-than-light speeds, they would be surprising but not a major crisis. His work passes the predictiveness test. Cooper was asking Segrè to repeat the experiment with tachyon detection in mind. He claimed that the odds of detecting tachyons were good, but not one hundred percent. If they were detected or not, his hypothesis would either be proven or falsified. We are taught at a young age that part of the scientific method is that the experiment be reproducible at other labs. Real science does not always work that way, since the experiments are often far too cumbersome for others to repeat, not to mention that some scientists, such as astronomers and paleontologists, do not do experiments. However science actually operates, Cooper seemed to be on the side of the traditional scientific method. We have in Cooper a clear nut who managed to pass for a legitimate outsider scientist: he was published in a physics journal.

CONCLUSION: USEFUL OUTSIDERS AND UNHELPFUL NUTS

Alvarez's Nutfile and his scientific career outside physics provide an opportunity to explore the borderlands of science, as well as non-science. Michael Shermer's argument that the borderlands of science are a spectrum instead of a demarcation line is helpful, but it is also helpful to note the difference between an insider, an outsider, and a nut. Perhaps this implies a spectrum with two axes, one on the insider-outsider axis and one on the good science-nut axis. Without indulging in such technicalities, it may suffice to understand what makes a nut and what makes an outsider. A person with a hypothesis can be one or both or neither. Alvarez's search for monopoles may have struck some scientists as a waste of money, but it was not bad science. Alvarez was often an outsider to a specific discipline, but never a nut. Even when he was an outsider to paleontology, he was never an outsider to science. He was an outsider to forensics, but his work on the Kennedy assassination has held up over time. Alvarez was obviously not a nut.

Alvarez argued that being an outsider could be beneficial. His team's approach to the dinosaur extinction certainly brought new techniques to the field of paleontology.

While he often derided physicists who had no experience with radio for their inability to solve obvious electronics problems, he also described a clear example of the useful outsider. When he worked on early radar systems during the Second World War, he developed Ground Control Approach (GCA), which used radar to guide an airplane down in zero visibility. That system needed a much finer resolution than current radars, so Alvarez tried to use a radar dish with two hundred dipoles, the part of the antenna where the radio waves are supposed to converge. Radio engineers told him,

> You can't make an antenna with so many elements in it, because if you try to put two together it's okay; if you put three or four together it gets very difficult, and you try to put ten together like we have done in some lower frequency arrays, it gets terribly difficult to tune them all up' and if you go beyond that, it's impossible.[169]

Alvarez was not thinking like a radar engineer; he was thinking of his graduate school training in optics. He had a lot of experience with diffraction gratings[170] and reasoned that this supposedly impossible problem would solve itself if he thought of it as a diffraction grating instead of as multiple radar dipoles. He was right: GCA worked beautifully, earning Alvarez the 1945 Collier Trophy, the annual aviation trophy. In another example from his WWII radar work, Alvarez noted that,

> The British had found that nuclear physicists had greater aptitude for pulsed radar development because we made our living with electronic pulses from ionization chambers and Geiger counters. Radio and electronic engineers, who

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[169] Charles Weiner and Barry Richman interview with Alvarez, 14 February 1967 (Session I), p. 26.
[170] A diffraction grating is a metal plate with thousands of parallel lines etched onto the surface. When light waves hit the surface, they interfere with each other and different colors are reflected at a different angle. He created an interesting analogy: the radio engineers were trying to solve three- and ten-body problems, while Alvarez thought of the system in terms of thermodynamics. Alvarez's first scientific paper was one he published as an undergraduate using a broken record as a diffraction grating: Alvarez, "A Simplified Method for the Determination of the Wave Length of Light," *School Science and Mathematics* 32, no. 1 (Jan. 1932), 89.

> were more familiar with the quasi-sinusoidal signals used in audio or radio-frequency engineering, abhorred pulses; such signals usually indicated trouble—lightning flashes or sparking.[171]

Radar is, of course, a type of radio work, so the experience of radio engineers should have given them an advantage. Instead, being trained to avoid pulses was a disadvantage. As we have seen, Alvarez's scientific career outside of physics was very fruitful precisely because he thought like an outsider.

Does that mean that one of Alvarez's nuts could be the next Einstein? It is unlikely, but the Cooper story illustrates that a nut who passes Baez and Bernstein's tests is best handled not by rejecting him out of hand, but by judging his arguments, as Allan Franklin did. Franklin wisely chose not to argue with Cooper on his terms—the statistical analysis of the pion time-of-flight—but instead to judge his conclusions. What if one of the nuts is right? If his theory fails the correspondence test, then it is his theory against the foundations of physics, a battle he will not win. If he fails the test of predictiveness, then the theory has no worth—it is unscientific in the sense that it does not perform the job that science performs. If it scores high on Baez's Crackpot Index, then it could, in principle, be correct, but I think we can take our chances.

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[171] Alvarez, *Adventures*, p. 87.

Chapter 6: *The Whiteness of Luis Alvarez*

"I have never thought of myself as being either Spanish, or Spanish-American, and I will certainly be no part in your scheme to present me to minority peoples as a member of a minority," wrote Nobel Prize winner Luis Walter Alvarez in 1973 to a Carnation Company biographer who wanted to frame him as a minority scientist.[1] Alvarez pledged his "non-cooperation in a program that I consider to be a 'phony' by any definition I know." His inclusion in a biography of "fifty-two contemporary Hispanic Americans"[2] would have been a stretch—he was as Irish as he was Spanish. Alvarez biographer Richard Rhodes said, "He couldn't have looked more Anglo-Saxon. He was ruddy."[3] The Carnation Company's lawyer apologized, explaining that Alvarez's Spanish surname was the only reason for the mistake. Including a Spanish-surnamed physicist would seem an innocent mistake, but Alvarez's strong, negative reaction raised questions about identity and blurred categories. This story provides fascinating anecdotal evidence to help understand the ethnic boundaries of Hispanic-white identity.[4]

Physics resists discourse with race. While the social sciences probe race, class and gender, the international communities that comprise the physical sciences maintain

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[1] Luis W. Alvarez, to Ms. Betty Logas, 10 September 1973, LWAP, box 17, folder 4, "C 1971-74 [2 of 2]." It is not clear who Ms. Logas is, but the letter is addressed to the Carnation Company in reply to Atkinson's letter. She was probably a company representative or secretary. It is not at all clear why this particular company was doing biographies of minorities in science.

[2] J. Edward Atkinson, Senior Public Relations Supervisor for the Carnation Company to Alvarez, 27 June 1973, LWAP, box 17, folder 4, "C 1971-74 [2 of 2]."

[3] Richard Rhodes interview by author, 20 May 2009, p. 37.

[4] I have contacted one of Luis Alvarez's sons and attempted to contact another son, Berkeley geologist Walter Alvarez. Neither have been willing to give an interview or answer questions about their father.

that physics is not racialized. The fact that science has facilitated European colonization is ignored: medicine led to malaria cures; the chemistry of explosives and steel led to the machine gun; physicists working on thermodynamics helped develop the steam engine. Historians Daniel Headrick and Jared Diamond have written about the impact these technologies have had on colonization.[5] Science in the United States and Europe—the two areas that have largely dominated science—has largely been a white, male endeavor.[6] But what do elementary particles have to do with race, class, or gender? Moreover, why should it matter that Alvarez was white? Some might argue, as affirmative action opponent Richard Rodriguez might, that no Nobel Prize winner can be considered a minority. Even if Alvarez had been from a poor Chicano family, his education and career would have distanced him from his ethnicity. The fact that his family had been well educated for at least two generations strengthens Alvarez's argument about a "phony minorities in science" biography. Yet Alvarez's personal papers reveal much more of the story. They reveal a Spanish-surnamed white man who is simultaneously hypersensitive to being called a minority and yet sometimes regretful that he is not closer to his partly Spanish ancestry. Further, the history reveals a Latino press that is hungry for Latino scientists, starving for role models. These imperfections—these fault lines—in Luis

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[5] See Daniel Headrick, *Tools of Empire*. (New York: Oxford University Press, 1981) and Jared Diamond, *Guns, Germs, and Steel* (New York: W.W. Norton, 1997). Another, more general treatment of race and science is Sandra Harding, ed., *The "Racial" Economy of Science: Toward a Democratic Future*. (Bloomington: Indiana University Press, 1993).

[6] This chapter focuses on the "minorities in science" approach. For women in science, see Margaret Rossiter, *Women Scientists in America: Struggles and Strategies to 1940* (Baltimore: Johns Hopkins University Press, 1982) and *Women Scientists in America: Before Affirmative Action, 1940-1972*. (Baltimore: Johns Hopkins University Press, 1995). For an interesting discussion of Jews in American physics, Daniel Kevles includes a little bit of coverage in his *The Physicists: The History of a Scientific Community in Modern America* (Cambridge, Mass.: Harvard University Press, 1995).

Alvarez's life and career suggest that there is more to unravel, that achieving whiteness is neither the solution nor the inevitability that Richard Rodriguez insists.

White Americans do not often talk about their whiteness. In Alvarez, we have an exception. Rhodes told me about the white scientist with the Spanish name: "because of the anomaly I think he probably did talk about it. He certainly did with me."[7] As we shall see, many have written about minorities in science, but Alvarez's willingness to talk about his race means he provides us the rare opportunity to investigate whiteness in science. Rhodes continued, "I don't remember that it was something I had to probe for. … It amused him that he was this tall ice blonde but his grandfather was Spanish." Had Alvarez identified as a minority, he might have become a great exemplar of minorities in science, or he might have shied away from discussing his ethnicity. While a doctorate aided such African American leaders as W. E. B. DuBois and Martin Luther King, Jr. in their quest for racial equality, African American biologist E. E. Just wanted simply to be known as a biologist, not a black biologist.[8] Being a famous minority physicist might have diminished Alvarez's achievements, which might have become "pretty good for a Latino." Instead, we have in Alvarez an American physicist who was intensely interested in developing his identity as a physicist while also addressing his whiteness. This is a rare opportunity that must not be wasted.

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[7] Rhodes interview by author, 20 May 2009, p. 36.

[8] Kenneth R. Manning, *Black Apollo of Science*. (New York: Oxford University Press, 1983).

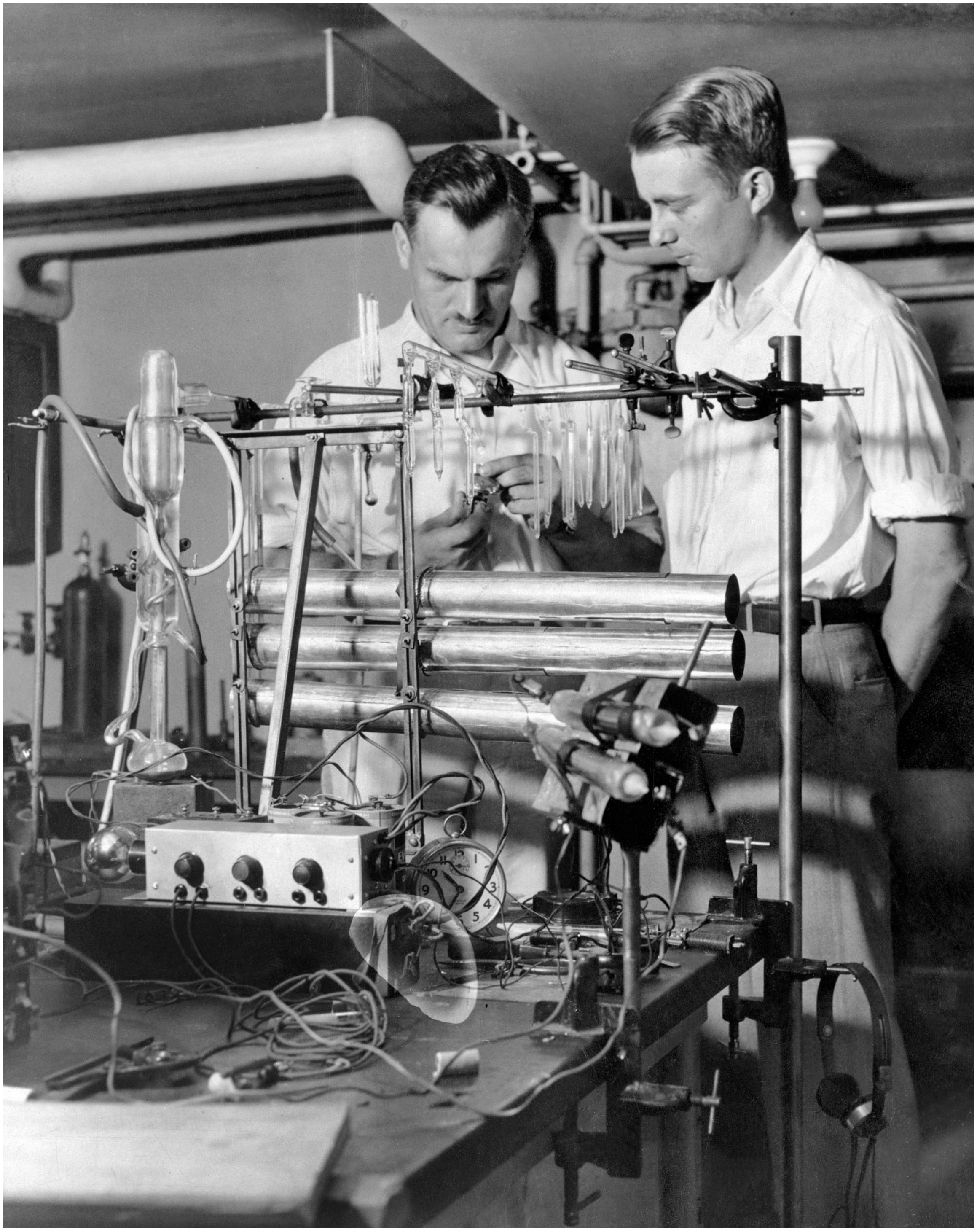

Figure 6.1: Graduate student Alvarez (right) with Arthur Holly Compton, 1933.

WHO IS HISPANIC?

A discussion of Alvarez's ethnic or racial identity will require a look at racial and ethnic definitions. The most respected books on the history of Mexican-Americans often punt on the issue of explaining terms and books in whiteness studies do not seem to believe they need to define whiteness. Mario T. García, in his respected *Desert Immigrants*, and George J. Sánchez, in his equally well-received *Becoming Mexican American*, relegate definition of terms to a footnote. García's definitions are arguably simplified:

> In this study the term *Mexican* refers to all persons of Mexican descent but in particular immigrants from Mexico. The term *Mexican American* refers to Mexicans born in the United States. The term *Spanish surnamed* refers to all persons of Mexican descent. Finally, *American* or *Anglo* refers to all non-Mexicans, especially white citizens of the United States.[9]

García was exploring the history of El Paso, Texas, so his definitions might make sense in that context, but it is incongruous that Alvarez would not qualify as "Spanish surnamed." Sánchez, who was writing about Los Angeles, might be expected to come a little closer to Alvarez's San Francisco upbringing. However, in a footnote, he notes the debate over labeling and says that he "will add little toward a resolution of this problem. … I have chosen to simplify for the sake of clarity a rather complex and politically charged issue."[10] He provides these somewhat lengthy definitions:

> Those born in Mexico who reside temporarily in the United States are called "Mexican," "Mexicano," and "Mexican immigrant" interchangeably.

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[9] Mario T. García, *Desert Immigrants: The Mexicans of El Paso, 1880-1920* (New Haven, Conn.: Yale University Press, 1981), p. 2, unnumbered footnote.
[10] George J. Sánchez, *Becoming Mexican American: Ethnicity, Culture and Identity in Chicano Los Angeles, 1900-1945* (New York: Oxford University Press, 1993), p. 277n1.

> "Mexican American" denotes both those born in the United States and those who change their citizenship status. I also use "Chicano" as an umbrella term for both groups, although I am aware that most of the individuals described in this study would not have used this term to describe themselves. "Latino" is used to describe the entire population of immigrants from Latin America and their descendants.[11]

Alvarez confronted every one of those terms, either in requests for biographies or in self-describing what he was not.

Sánchez did not stop there; he explained what he meant when he used terms for whites as well, albeit without much analysis. He continued in the same footnote as above:

> In this study, I will also use the terms "white," "Anglo American," and "Euro-American" interchangeably, although I also acknowledge the great diversity existing among those so designated. When appropriate, I refer to the national origins of subgroups within this "Anglo American" population.[12]

Alvarez would, in this rubric, be a "Euro-American," "Anglo American," and "white," since he was of European descent. Although he did not have any Anglo-Saxon heritage, it is common in the literature of Chicano studies to refer to all non-Hispanic white Americans as Anglo, as in the title of David Montejano's *Anglos and Mexicans in the Making of Texas*. Montejano avoided the debate over labeling by noting that Mexicans could be defined as a nationality, ethnicity, or race, arguing that "One of the better known axioms in the social sciences is that 'races' are social definitions or creations."[13] By using a political definition, he hoped "to sidestep a historical argument about the origins of Anglo-Saxon prejudice—whether these attitudes were imported and transferred to

[11] *ibid*.
[12] *ibid*.
[13] David Montejano, *Anglos and Mexicans in the Making of Texas, 1836-1986* (Austin: University of Texas Press, 1987), p. 4.

Mexicans or were the product of bitter warfare."[14] As we shall see, is a good strategy because the definitions would often shift, even if Alvarez himself always maintained that he was not Hispanic.

In acknowledging that the definition of "white" needs to be addressed, Sánchez and Montejano foreshadowed the rise of whiteness studies. David Roediger argued that even in the absence of blacks or other minorities in his hometown, race remained a constant fact of life: "I learned absolutely no lore of my German ancestry and no more than a few meaningless snatches of Irish songs, but missed little of racist folklore."[15] Roediger described the racist mythology, language, and politics of a town with no one to discriminate against in the introduction to *The Wages of Whiteness*, which has spawned a literature on whiteness. He explained that "the term *white* emerged as European explorers, traders and settlers who came into contact with Africans and the indigenous people of the Americas," implying that white simply means European.

Neil Foley added to this literature, analyzing Texan agricultural history in terms of white, black, and Mexican. He notes that the nomenclature is full of contradictions, but notes that "central Texas Czechs and Germans, who spoke different languages and often attended different churches and schools, still thought of themselves as whites when they were in the company of Mexicans and blacks."[16] Foley argues that the development of a poor white population in the South was a challenge to racial sciences such as

[14] *ibid*., p. 5.
[15] David Roediger, *The Wages of Whiteness: Race and the Making of the American Working Class* (London: Verso, p. 1991), p. 3.
[16] Neil Foley, *The White Scourge: Mexicans, Blacks, and Poor Whites in Texas Cotton Culture* (Berkeley: University of California Press, 1997), p. 9.

eugenics. The solution was to create a new category, "white trash," for elites to explain why people with supposedly "good" genes were not prospering economically. In a striking example of culturally defined concepts of race, white elites made "white trash" its own race so that wealthy whites could still claim that genetics naturally resulted in their success. This chapter hopes to continue in the tradition of whiteness studies rather than the "minorities in science" tradition that tried to co-opt Alvarez.

Alvarez was aware of the significance of this issue even though he could sometimes seem callous about it.[17] He was probably sympathetic to minority issues, or certainly wanted to give the impression that he was, as we shall see. Although his politics were most well known for pushing for more nuclear weapons research, he was a Democrat. Dictating his autobiography in May of 1972, Alvarez noted that

> These days, "Spanish surnames" are important—in a recent University-wide survey, the Berkeley physics department came out with good grades because my name was first on that list, alphabetically.[18]

He could not speak Spanish and suggested that he "would be a more credible Chicano if I could speak Spanish with a Mexican accent."[19] In May 1980, in a reply to a request from the American GI Forum, the Hispanic veteran and civil rights organization, Alvarez noted that "the Physics Department is not permitted to claim that they have a Hispanic faculty member."[20] Apparently, something happened between 1972 and 1980. It seems as if, despite Alvarez's sometimes angry objections, his ethnicity was sometimes up for grabs.

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[17] He was aware of that impression, as well. Alvarez autobiography draft dated 5 Jun 1972, LWAP box 46, folder "Autobiography p. 1-100," p. 100.

[18] Alvarez autobiography draft dated 5 Jun 1972, LWAP box 46, folder "Autobiography p. 1-100," p. 4.

[19] *ibid*.

[20] Alvarez to J.Q. Rodriguez, Convention Chairman, American GI Forum Education Foundation of Kansas, 1 May 1980. LWAP, BOX 39 folder "O 1978-87." Alvarez spelled Rodriguez "Rodriques."

The historical record has not given up its secret on why the Berkeley survey changed its policy on Alvarez, but it has exposed a great deal about Alvarez's whiteness and its place in physics.

WHITE HERITAGE

As it turned out, Luis Alvarez's genealogy lent itself to either identity—Anglo or Hispanic. However, his identity was not questionable. Being one-quarter Spanish, he could have chosen a Hispanic or Spanish American identity without giving up his white status. Spain is in Europe and Europeans are the definition of white. Many New Mexico Hispanics consider themselves white. But Alvarez was from California and he seemed to feel the need to take a strong stance on his ethnicity. White Hispanic, an option on today's census forms, was not good enough. Spanish American was no better, but he was correct to question attempts to concentrate on his paternal grandfather. Had his mother's mother been Spanish rather than his father's father, he would be no less Spanish, but no one would suspect that a Dr. Louis Smyth was Hispanic.

Luis W. Alvarez's only hereditary link to his Spanish surname came from his paternal grandfather, Luis F. Alvarez, a Spaniard from Asturias described as "short, blond, blue-eyed, and slight in build."[21] Photographs demonstrate that Luis's grandfather could have passed for German. Luis's father, Walter, credited the Visigoth invasion of Spain for his father's racially motivated work ethic. Because of this invasion, Walter

[21] Walter C. Alvarez, *Incurable Physician: An Autobiography* (Englewood Cliffs, N. J.: Prentice-Hall, 1963), p. 22. This description is from the first autobiography of Luis's father, the medical doctor well known for his weekly column on health. I will include his middle initial to distinguish him from Luis's son, also named Walter.

believed that the Spaniards of this region were "blond, energetic, and hard-working—more like Germans than are the often easy-going peoples of the South of Spain."[22] That Walter associated a work ethic with Germanic blood coincided with his chosen identity. Apparently, even Luis's father tried to de-emphasize his Spanish heredity despite being fluent in the language. Luis W. Alvarez said he only talked to his grandfather Luis F. Alvarez "on a few occasions, in his later years" and "found no hint of a foreign accent."[23] Luis F. Alvarez moved to Los Angeles after some time in Cuba and New York, became a doctor, and "gained some measure of prosperity, since … in the early 1880's, he owned one block of downtown Los Angeles."[24] Luis W. Alvarez continued,

> He had apparently been a successful investor, but his estate was greatly reduced in value when some South American countries later defaulted on the bonds which made up a substantial part of his portfolio. In 1929, he was in his late 70's and could have retired to a life of ease. But he went every day to his [doctor's] office in the poorest section of the Spanish-speaking district in Los Angeles. He thought that these people needed him, and they obviously had great admiration and affection for him.[25]

The elder Luis was Spanish but blond, wealthy but sympathetic to the plight of the poor. If he was not white enough, Walter's maternal genealogy provided further support.

Walter's mother, Clementina, had a Danish father and a German mother by way of Minnesota. Walter explained that this was "Perhaps another reason I am tall and blond, instead of short and swarthy."[26] He says that his father married Clementina "partly because she looked so Spanish and partly because of their natural attraction for each

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[22] Walter C. Alvarez, *Incurable Physician*, p. 21.
[23] Alvarez autobiography draft dated 14 May 1972, LWAP box 46, folder "Autobiography p. 1-100," p. 5.
[24] *ibid*.
[25] *ibid*, p. 56.
[26] Walter C. Alvarez, *Incurable Physician*, p. 21.

other."[27] With half of his roots spread across Berlin and Minnesota, Walter could have been justified in identifying as Anglo.

Luis W. Alvarez's mother, Harriet Smyth, grew up in China. Her "parents, Dr. and Mrs. George B. Smyth, were the founders of a missionary school in Foochow, China—the Anglo-American College."[28] The name seems to say it all. Harriet's father was born in Ireland before moving to Minnesota and China. Luis W. Alvarez said he remembered "little of my maternal grandmother and nothing of my grandfather."[29] Walter C. Alvarez "grew up in almost complete isolation from white companions in the village of Waialua," Hawaii. With parents raised in China and the Kingdom of Hawaii, Luis W. Alvarez grew up "isolated from the knowledge of athletics in my early years, since I was brought up by a father who couldn't tell a baseball player from a football player, and a mother who had not been brought up among native Hawaiians, but instead among native Chinese."[30] In a surprising way, Luis W. Alvarez spent his first few years in San Francisco as an outsider to American culture, not as a Hispanic, but as someone influenced by Chinese and Hawaiian culture.

Walter took over his father's practice from 1907 to 1909 as town doctor for the northern Mexican city of Cananea.[31] San Francisco was still in ruins from the 1906 earthquake and local medical schools could only offer Alvarez a room and twenty-five

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[27] Walter C. Alvarez, *Alvarez on Alvarez* (San Francisco: Strawberry Hill Press, 1977), p. 4.
[28] Alvarez autobiography draft dated 14 May 1972, LWAP box 46, folder "Autobiography p. 1-100," p. 4.
[29] Alvarez, *Alvarez: Adventures of a Physicist* (New York: Basic Books, 1987), p. 10.
[30] Alvarez autobiography draft dated 14 May 1972, LWAP box 46, folder "Autobiography p. 1-100," p. 6.
[31] These years in Mexico are described in Alvararez, *Adventures*, p. 10, Walter C. Alvarez, *Alvarez on Alvarez*, pp. 37-41, and Walter C. Alvarez, *Incurable Physician*, chapter 4.

dollars a month, while the job in Cananea paid well.[32] These three years shaped his early career and defined his impression of Mexican ethnicity in terms of class. Cananea was a small mining town twenty-five miles south of the Arizona border controlled by the American Cananea Consolidated Copper Company. The Cananea Company hired skilled American professionals and paid them higher wages, in dollars, than the Mexican laborers who were paid in pesos. Besides having low paying jobs, Mexicans had no option but to purchase everything from a company store with highly inflated prices. The engineers and doctors at Cananea were all American—"Mexican professionals were not accepted."[33] Just before Walter moved there, workers went on strike against their American bosses in 1906.[34] This strike "unchained the need for the revolution that culminated four years later with the uprising of Francisco Madero and his supporters" who started the Mexican Revolution.[35] The strike is celebrated for its place in labor history and for contributing to the end of Porfirio Díaz's long dictatorship. Although not as directly related, it was also inspiration for Mexico's 1938 expropriation of American companies. Race and class were in stark contrast for Walter.

Walter came from an upper-middle class background to become the town doctor for Cananea's poorly paid miners. Walter's light complexion could have helped him fit in at elite European affairs, but instead he worked among lower-class Mexicans as the

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[32] Walter Alvarez, *Incurable Physician*, p. 43.
[33] Manuel Arellano, *Huelga de Cananea 1906*. (México, D. F.: Libros de México, 1976), p. 7. Translation by author.
[34] The 1906 strike in Cananea is best described in Manuel González Ramírez, *La Huelga de Cananea* (México, D.F.: Instituto Nacional de Estudios Históricos de la Revolución Mexicana, 2006). However, two small pamphlets from Arellano and Calderón contain succinct and very quotable passages.
[35] Esteban Baca Calderón, *Cananea* (México, D.F., SEP/Conasupo, 1980), p. 2. Translation by author.

town doctor. This must have shaped Walter's interpretation of his time in Mexico. It was this interpretation that would later influence Luis Alvarez and help mold his identity. Surely, Walter must have had an impact on Luis's expectations for his trip to Mexico in the 1930s.

Although Walter C. Alvarez made a point of stating that the Mexican people were kind and generous, he also took time to criticize their simple naïveté concerning medicine.[36] Apparently he had little of the sympathy for the poor that his father had. In his first autobiography, *Incurable Physician*, he devoted six of his seven pages about Cananea to describing the backwardness of the Mexican workers. His experience in Mexico was full of stories about hysteria, superstition, a man threatening Walter with a .45 revolver if he delivered a baby girl, and a case of a man eloping with his mother-in-law.[37] His opportunity to return to San Francisco late in 1909 was his "miraculous escape" from this "intolerable situation in Mexico."[38] Walter described the day he received the invitation to return to San Francisco as "One of the happiest days of my life."[39] Obviously, Walter had no opportunity or desire to identify with his Mexican patients.

Although he spoke Spanish and had a Spanish surname, Walter Alvarez was not seen as Mexican, but as an outsider physician paid a good salary in American dollars. He certainly was not one of the oppressed workers, and this contrast must have been

[36] Walter Alvarez, *Incurable Physician*, chapter 4, "Mining Camp Doctor." For some reason, *Alvarez on Alvarez* was much more kind to his Mexican patients.
[37] *ibid*., chapter 4, pp. 43-49.
[38] *ibid*., p. 50.
[39] *ibid*.

noticeable. Luis W. Alvarez developed an even more cynical view than his father. Walter Alvarez was only there for a short "three years in the Wild West," as his son Luis called it.[40] Walter must have seen the obvious class divisions—they were probably the source of his repulsion for the experience. These three years certainly did not become an opportunity for the half-Spanish Walter to identify with Mexicans. He was an outsider in Mexico, due in large part to the class differences between him and his patients. If anything, the experience pushed him away. He likely passed these beliefs on to his son.

The experiences of Walter Alvarez were fundamental to understanding how Luis Alvarez developed his politics and identity. Coming from a fair skinned, blond family gave him the racial mobility of a "tall, ruddy blond,"[41] which allowed him to thrive among white scientists. Luis W. Alvarez reported that his father, Walter, was politically conservative,[42] and it is clear that his class position helped form a barrier between himself and working class Mexicans in Cananea, and likely kept him at a doctor-patient distance with California Mexicans. This class position was reinforced by its vintage. The Alvarez family had been well off for at least three generations before Luis.

Luis Walter Alvarez was born in San Francisco in 1911, two years after Walter's return from Cananea. He had missed being born in Mexico, as his older sister Gladys had been. Next came Robert in 1912 and Bernice in 1913. Luis became the only child with a Spanish-spelled name like his grandfather's—although his brother Robert could go by

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[40] Alvarez, *Adventures*, p. 10.
[41] *ibid.*, self description on p. 9.
[42] Alvarez autobiography draft dated 5 Jun 1972, LWAP box 46, folder "Autobiography p. 101-200," p. 105.

Roberto, his sisters Gladys and Bernice had distinctly Anglicized names. However, Luis became "'Sonny,' as much for his good disposition as for his family status."[43] Luis Alvarez got an early start avoiding the Spanish spelling of his first name with a characteristically Anglicized childhood nickname.

Walter Alvarez and his wife, Harriet, both spoke fluent Spanish. Luis Alvarez told would-be biographer Philip Carona that his son, Walter, and his wife "both speak fluent Spanish, from having lived two years in Colombia."[44] As if the language barrier were a major impediment for this world traveller, Luis said that "My mother and father, and my son and daughter-in-law who are all fluent in Spanish, have visited the old family farm, and I have corresponded with my Spanish relatives through an interpreter."[45] Luis Alvarez was somehow left out, the only generation of Alvarez who did not speak Spanish. Luis lamented his parents' choice:

> Unfortunately they did not teach the language to me, but instead used it as a "secret language" so that they could talk without having their children understand what they were saying on special occasions.[46]

In a letter to a University of Madrid physicist, Alvarez adds that he was "most unhappy" about his parents' choice to use Spanish as a "secret language."[47] In a passage from his autobiography draft, he says, "I'm still a bit annoyed with my mother and father, who

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[43] Daniel Wilkes, "Luis Alvarez—Restless Spirit of Science" manuscript, LWAP, box 5, folder 6, "W 1956-64 [1 of 2]," p. 5.

[44] Alvarez to Philip B. Carona, 12 September 1972. LWAP, box 17, folder 3, "C 1971-74 [1 of 2]."

[45] Alvarez autobiography draft dated 14 May 1972, LWAP box 46, folder "Autobiography p. 1-100," p. 4.

[46] Alvarez to Inocencia Alvarez, 21 February 1964. LWAP, box 1, folder 1, "A 1956-64." Inocencia Alvarez of Montevideo, Uruguay wrote to Luis Alvarez after corresponding with Luis's father, Walter, about a meeting with her uncle. Luis and Inocencia agree that they may be a distant relation, but are unsure.

[47] Alvarez to Nicolas Cabrera, 24 May 1976. LWAP, box 26, folder 6, "Correspondence 1976 [Outgoing]."

used Spanish as a 'secret language,' when they could so easily have given me and my siblings the advantage of being bilingual."[48] That sentence, originally in parenthesis, was crossed out by hand and reworded to omit "a bit annoyed," as if he wanted to keep family tension out of the public record. Alvarez realized an interesting hypothetical: "If I had learned [Spanish] from my mother, I would now speak with a Mexican accent, since she learned the language in a Mexican mining town."[49] He realized that this language of paternal secrets distanced him from his parents and his Spanish grandfather without compromising his career in physics, since most important research in particle physics in the twentieth century was published in English or German.[50]

Luis grew up in San Francisco, where he must have had a fairly sheltered existence. He wrote that at summer Boy Scout camp, "I became acquainted for the first time with what would now be called underprivileged boys; everyone I had associated with prior to this time would now be referred to as upper middle class."[51] In 1926, when Alvarez was fourteen or fifteen, his family moved to Minnesota where his father became a researcher at the Mayo Clinic. The new background helped him come out of his social shell:

> In high school in California, I had never visited the home of a friend. With the exception of one party none of my friends had visited me. In Minnesota we were in and out of each other's houses day and night, girls and boys. … I felt

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[48] Alvarez autobiography draft dated 14 May 1972, LWAP box 46, folder "Autobiography p. 1-100," p. 4.
[49] *ibid*.
[50] "Language of paternal secrets" comes from Claudio Segrè because of the parallel between Alvarez and Segrè, a son of Italian immigrants, as well as the link to Richard Rodriguez's concept of public spaces. Claudio Segrè, *Atoms, Bombs, and Eskimo Kisses* (New York: Viking, 1995), p. 25. Claudio was the son of Emilio Segrè, a great Italian-born physicist who worked at Los Alamos and Berkeley contemporaneously with Alvarez.
[51] Alvarez autobiography draft dated 14 May 1972, LWAP box 46, folder "Autobiography p. 1-100," p. 14.

> like a country cousin at first, but since I had been to dancing school in California and knew how to dance my new friends regarded me as something of a city slicker. If I had remained in San Francisco, I think I would be a different person.[52]

Although he would not likely have interacted much with minorities in San Francisco, he might have been a different person if he had at least been exposed to minorities in San Francisco instead of the waspy Minnesota.

In this new high school, "Luis was the tall, clean-cut, All-American Boy type."[53] Luis's father, Walter, remembers his high school career as non-conformist. Perhaps because of Luis's ability in "showing his principal what some of these stupidities [of high school education] were," he never made it on to the honor roll.[54] Walter wondered "whether [Luis's] teachers would have lifted a finger to help him go to college. … if I had died and left Luie penniless"[55] Clearly, Luis Alvarez was not a disadvantaged youth; instead, he had an educated, respected doctor for a father who looked out for his son.

THE BIOGRAPHERS

Possibly as important to understanding Alvarez's own identity is how others categorized him. Authors who agreed with him politically had no trouble getting permission to do biographical essays. The strongest clues we have to the nature of Alvarez's whiteness came when writers approached him for permission to write "Hispanic Physicist"-type biographies. They varied in their approach and their

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52 Alvarez, *Adventures*, p. 13.

53 Wilkes, "Luis Alvarez" manuscript, p. 9.

54 Walter Alvarez to Daniel Wilkes, 11 January 1958. LWAP, box 5, folder 6, "W 1956-64 [1 of 2]," p. 4.

55 *ibid*.

aggressiveness. Not surprisingly, these biographies only succeeded when Alvarez was unable to refuse their stories.

In 1972, Philip Carona, a Houston public school teacher, wrote to Alvarez asking for help in developing something like "a multi-media package directed towards children in Grades 6-8."[56] Carona explained, "Dr. Alvarez, you are certainly one of the world's most prominent Mexican-Americans. No program would be complete without your story."[57] He continued,

> Dr. Alvarez, I realize that this is quite a favor I am asking of you, but my intentions are very good. I should like to play a part in making a contribution that is greatly needed by our children. I have the desire. I am willing to exert all necessary efforts. I merely need this beginning force of personnel location. Please help me.[58]

No doubt Alvarez would have been an excellent candidate had he been Mexican-American. There were few Mexican American scientists at the time and certainly none with Alvarez's standing in the scientific community. However, Luis Alvarez was unable to help since, as he pointed out,

> I am not one of the world's most prominent Mexican-Americans. It is really quite an accident that I have a Spanish name. … Neither my grandfather nor any of my other relatives has ever had any relationship with Mexico.[59]

His grandfather and father's time as doctors in northern Mexico notwithstanding, Luis Alvarez was as open, courteous, and helpful to Carona as he could be.

Alvarez was cordial to inquiries. After winning a Nobel Prize in 1968, it is surprising that he would respond to these requests at all. In 1980, J.Q. Rodriguez wrote

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[56] Philip B. Carona to Alvarez, 30 August 1972. LWAP, box 17, folder 3, "C 1971-74 [1 of 2]."
[57] *ibid.*
[58] *ibid.*
[59] Alvarez to Philip B. Carona, 12 September 1972. LWAP, box 17, folder 3, "C 1971-74 [1 of 2]."

asking if Alvarez could speak at a conference for the American G.I. Forum Education Foundation of Kansas, an "organization which provides scholarships and other financial aid, counseling and tutoring, and specialized assistance for Vietnamese and Laotian refugees and other people with language difficulties."[60] He wrote to Alvarez because he thought Alvarez could "share with us the benefits of your experience." They "would be inspired by the example of your life, and the convention delegates would greatly appreciate your sharing of ideas and your assessment of the needs and opportunities of the Hispanic community."[61] Alvarez replied courteously that he would be "at a conference in Massachusetts at that time," which would have been enough to settle the issue, but he added that "I am sorry to say that I am not 'Hispanic.' My grandfather came from Spain—not Mexico."[62] He kept his reply short, and left no space to explain why a Spanish grandfather made him *not* Hispanic. Having roots in Spain is essentially the definition of Hispanic, but in California in 1980, Hispanic usually meant Mexican. Rodriguez never mentioned Alvarez being Mexican, only Hispanic. Perhaps Alvarez did not have the time to explain, but his short description raised more questions than it answered.

Alvarez replied even more succinctly to Thomas Odom, chairman of the American Association of Physics Teachers' committee on Physics in Minority Education. The entire reply reads: "Thank you for the invitation. But (1) I am not a member of a

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[60] J.Q. Rodriguez, Convention Chairman, American G.I. Forum Education Foundation of Kansas to Alvarez, 1 May 1980. LWAP, BOX 39, folder "O 1978-87."
[61] *ibid*.
[62] Alvarez to J.Q. Rodriguez, 6 May 1980. LWAP, box 39, folder "O 1978-87."

minority group and (2) I can't be in San Antonio for the meeting."[63] Odom included a potential list of speakers on minorities in physics: "We envision a panel consisting of you, Drs. Robert Ellis of Plasma Physics Laboratory, Princeton, Ronald McNair of NASA and Walter E. Massey of Argonne National Lab."[64] None of those other potential panelists carried the prestige of a Nobel Prize winner like Alvarez, but Ronald McNair—an African-American astronaut—did later attain some unfortunate notoriety as one of the astronauts who died aboard the Space Shuttle Challenger in 1986. Odom's claims on the significance of his conference were substantial:

> It would not only make a positive impact in San Antonio and throughout the physics community but the ramifications would be thrusted throughout the minority factions, as well as other segments of this nation.[65]

This "thrusted ramification" never came to the other segments of this nation. Besides odd language, there were a few misspellings in the letter, but it was an earnest request that Alvarez treated with respect. He did not consider himself a minority and he was a very busy man.

Alvarez was not nearly so courteous when J. Edward Atkinson, Senior Public Relations Supervisor for the Carnation Company, asked for permission to include Alvarez in a book on fifty-two contemporary Hispanic Americans for the Carnation Company. Alvarez declined the offer, but Atkinson tried to change his mind:

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[63] Alvarez to Thomas W. Odom, Chairman, committee on Physics in Minority Education, American Association of Physics Teachers to Alvarez. LWAP box 39, folder "O 1978-87."
[64] *ibid*.
[65] *ibid*.

> Racial and cultural minorities usually suffered at the hands of the American majority, forced to change their ways and adapt to the Anglo mode of life. The Blacks, Indians and the Latin Americans have borne a heavy burden.
>
> It is clear today that we can no longer ignore minorities, and that we can no longer force them to pay the price for being different. We sense the need for greater understanding among all Americans. This book is a contribution towards this need.
>
> We feel that your biography belongs in this book.[66]

Included was a three-page biography by *Los Angeles Times* feature writer Al Martinez for Alvarez to approve. Not surprisingly, this request that Alvarez reconsider met with a strong reply: "I said I was against this because I was by no definition a member of a minority. I do not see how I could have made my position more clear than I did."[67] His argument was solid—he was not disadvantaged and had not struggled as the article assumed. However, it is most interesting to hear this white man express his whiteness in such clear terms. Not often does one run across a dialogue between two white people discussing their own ethnicity in the language of identity politics.

The Carnation Company's Robert F. Daily replied on the part of the legal department. He apologized and stopped the biography. He went on to explain, "This confusing surname phenomenon is typical of many Americans, as you know."[68] He conjectured that had his late brother, Alan Daily, "been the subject of a biography, many would have categorized him as an 'Irish-American,'" despite "our Irish surname

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[66] J. Edward Atkinson to Alvarez, 27 June 1973. LWAP, box 17, folder 4, "C 1971-74 [2 of 2]."
[67] Alvarez to Ms. Betty Logas, Carnation Company, 10 September 1972. LWAP, box 17, folder 4, "C 1971-74 [2 of 2]."
[68] Robert F. Daily to Alvarez, 19 September 1973. LWAP, box 17, folder 4, "C 1971-74 [2 of 2]."

representing only about 3/16 of our lineage."[69] This lawyer's attempt at getting Alvarez's sympathy raised an interesting question. Would an Irish-American physicist's work be categorized ethnically? It seems unlikely, unless, perhaps, he strongly identified as Irish Catholic. After all, Alvarez's maternal grandfather was born in Ireland and Daily was none the wiser. Why, then, was Alvarez's work up for ethnic categorization? This could provide clues to understanding whiteness and, specifically, what whiteness is in science.

"CHIPS" ALVAREZ

Colleagues saw Alvarez as a scientist, that is, as a white scientist, which, during the years Alvarez was active, was essentially the same thing. Prominent Hispanic scientists were all but unknown. Given Alvarez's reaction to being labeled a minority, one might guess that he appreciated this inclusion by white scientists. In a biography draft for *Reader's Digest*, Daniel Wilkes wrote that even as a graduate student, Alvarez "was accepted as a 'member of the club' by great scientists."[70] Wilkes most certainly intended that as a commentary on Alvarez's work, but it says a lot about Alvarez's identity.

To his colleagues and friends, Luis Alvarez was just another one of the boys. He earned the traditional carpenter nickname, "Chips," on sailing cruises with his college friends.[71] In college, his fraternity brothers gave him names such as "the mad Spaniard

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[69] *ibid*.
[70] Wilkes, "Luis Alvarez" manuscript, p. 15.
[71] Alvarez, *Adventures*, p. 17. Correspondence between Alvarez and Gordon Allen as late as 1971 demonstrate that the name stuck. LWAP, box 16, folder 4, "A 1971-74."

from Minnesota" or "the Spanish Swede,"[72] names that emphasized the juxtaposition of his Spanish surname with his Nordic appearance. Later, his colleagues would call him Luie, as opposed to the Spanish spelling of his given name, "Luis." Colleagues pronounced his name like "Lewis" instead of the Spanish Loo-EES. People who knew Alvarez knew about his identity as a white man and reaffirmed it with very non-Spanish nicknames. This may reflect their interpretation of a tall, blonde physicist who did not speak Spanish, or perhaps Alvarez directly encouraged this type of interaction. At times, Alvarez seemed to emphasize his lost connection to Spain and its culture. Besides his letters to biographers, he would also mention that he could not speak Spanish when the opportunity arose. When a friend, George Watkins, mentioned that he had visited Cuernavaca in Morales, Mexico, Alvarez replied: "I've frequently sat in the Plaza in Cuernavaca, and exercised my fluent Spanish to its fullest extent, by ordering 'dos cervesas.'"[73] Why could he not speak Spanish? Why mention his inability to speak Spanish? Did this help make him whiter?

ALVAREZ ON RACISM

In 1931, a nineteen-year-old Alvarez and his brother, Robert, went on many road trips to explore the country with a relish for travelling that would only grow in time. During spring break of that year, they checked off visiting every state east of the Mississippi by travelling through the Deep South. This was Alvarez's first exposure to severe racism and poverty and it may have helped him develop a politics that was

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[72] Wilkes, "Luis Alvarez" manuscript, p. 10.
[73] Alvarez to George A. Watkins, 18 April, 1977. LWAP, box 26, folder 3, "W 1975-77."

socially liberal, though he was a firm hawk on foreign policy and nuclear weapons. He recorded his thoughts on that trip in the early drafts of the autobiography that he dictated into a tape recorder. That dictation method seemed to open him up to talk about things many people consider taboo. Many people do not talk about whiteness the way Alvarez does, but while dictating, Alvarez opened up even more, so much so that his editor often blanched at his honesty. Alvarez's opinions on race did not make it into the final, published autobiography, but five pages of dictation on Alvarez's Deep South adventure were preserved in the autobiography draft at the Bancroft Library archives at the University of California, Berkeley after he died. The reminiscences of a white man born in 1911 on the topic of race might sound like a difficult topic that could be embarrassing to Alvarez or his family, but they cannot be ignored when trying to understand his ethnic identity and his views on his own race—views that happened to be strongly progressive. Alvarez was dictating in 1972, but his politics on race in America resemble mainstream racial attitudes today.

Alvarez began by explaining that in his early years in San Francisco—he moved away when he was fifteen—he did not see many black people:

> I remember once asking my mother what was wrong with a woman I noticed in Golden Gate Park, wheeling a baby carriage. My mother said, 'That woman is colored.' But I could tell that my mother didn't know what she was talking about—the lady wasn't colored—her skin was simply black!"[74]

San Francisco certainly has a population of blacks, but there were far fewer blacks there in the 1930s. Even today, it is a fairly segregated city with some very expensive, white

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[74] Alvarez autobiography draft dated 5 Jun 1972, LWAP box 46, folder "Autobiography p. 101-200," p. 101.

neighborhoods. Alvarez almost certainly lived in one of those upper middle class areas. Moving to Rochester, Minnesota in 1929 meant even less exposure to black people:

> I can't recall having ever seen a black person in Rochester the whole time I lived there. I remember hearing that the year before I came to Rochester there had been a black student who had been very popular, and in addition to being a star football player was also one of the top students in his class.[75]

Alvarez was sure that this story meant that, "although I had no contact with blacks during this period of my life, I had a 'favorable image' of them from this account."[76] One has to wonder whether having lived through the 1960s in Berkeley as a Democrat gave Alvarez a desire to sugarcoat his relationship to an oppressed minority. There is no reason to believe that he was a racist or that he was lying, but there is a sense of trying to impress the reader with his racial progressiveness. He described seeing "large numbers of black people for the first time" in Chicago, where he went to college and graduate school, adding that "It is obvious to me that I had no prejudice against them."[77] Editor Peter Trower underlined "no prejudice" and exclaimed, "This is a dangerous statement!"[78] It certainly would have been a striking statement had it survived to the published edition of his autobiography. Alvarez justified the claim by saying he had a black laboratory partner in Physics 263: "We had mutual respect, and picked each other for partners simply by unspoken mutual consent."[79] Alvarez certainly did not think he was racist and

[75] *ibid*.
[76] *ibid*.
[77] *ibid*.
[78] *ibid*.
[79] *ibid*., pp. 101-102.

odds are that he was simply being genuine. Perhaps racial equality was important to him and polite conversation did not afford him a chance to expound on those beliefs.

Alvarez says that he was not aware of "discrimination or injustice" against blacks before his trip to the Deep South. He suspects that he was young and had not started to develop a political mind of his own yet. He did notice, "driving down South Michigan Avenue, that the black citizens of Chicago lived in a very crowded manner, largely in run-down mansions in what had once been a very fashionable section of Chicago."[80] However, "I had just started to do my own independent thinking in science, and it would have been quite surprising if I had also begun to think independently in the field of sociology, as well."[81] He did allow the possibility that, "although I think it is not really likely, that I might simply have 'tuned it out' of my conscious mind."[82] So perhaps Alvarez is not claiming that he was entirely ahead of his time, but that he had no inclination towards racism and only noticed injustice if it was stark.

He was struck by the cultural differences between Yankee states and Confederate states on his trip. He described plaques honoring the "heroic soldiers who died to preserve the glory of the Confederate States of America," which clashed with his Minnesota high school history lessons that "had led me to believe that the Confederacy was led by evil men who were in insurrection against the duly constituted government of

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[80] *ibid*., p. 102.
[81] *ibid*.
[82] Alvarez autobiography draft dated 5 Jun 1972, LWAP box 46, folder "Autobiography p. 101-200," p. 102.

the United States and that they were properly punished for their wrong doings."[83] He was unprepared for this reverence, but said that he later learned to admire the heroes of the Confederacy as he learned more about the Civil War.

His trip through the Deep South provided that opportunity to witness stark racism, starting with segregation:

> I found it quite shocking to see all the signs on rest rooms and railway station waiting rooms, that distinguish between Men's toilet, white, Men's toilet, colored, Women's toilet, white; and Women's toilet, colored; and waiting rooms, white and colored.[84]

This is a natural reaction to travelling in the Deep South for the first time in the era before desegregation, but it again gets to Alvarez's reaction against racism. Here he is specifically claiming that it was shocking to him at the time, not just in retrospect while sitting in his office in Berkeley in 1972. It gets worse. After a tornado near Birmingham, Alabama, Alvarez and his brother wanted to investigate. He reports that "Perhaps my most vivid memory of the whole trip is of a comment made by a radio newscaster in a heavy southern accent, 'Fortunately no one was hurt, but several niggers were killed.'"[85]

Alvarez and Robert took the opportunity to look inside the destroyed shacks of a poor, black neighborhood, their contents strewn publicly after the tornado had destroyed any weak structures. It was Alvarez's first exposure to real poverty and an odd thing struck him: "There was very little in the way of furniture in the shacks, but I was impressed by the fact that a picture of Booker T. Washington seemed to be the standard

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[83] *ibid*., pp. 102-104. There is no page 103, as editor Peter Trower noted in handwriting.
[84] *ibid*., p. 104
[85] *ibid*., pp. 104-105.

wall decoration in the shacks we examined."[86] Perhaps Alvarez saw in those pictures a hope for self-improvement that countered stereotypes about the poor wanting handouts. He tied that experience to what he saw around him at Berkeley:

> When I watch the actions of black militants in the Bay Area these days, it is hard for me to realize that large numbers of men with essentially the identical genes lived under these conditions and were kept in a state of almost complete servility, some 40 years ago, and by the very real threat of lynching.[87]

Was Alvarez complaining that members of Oakland's Black Panther Party had it easy? Was he complaining that Southern blacks did not stand up for themselves? Was he making a statement about genetic determinism or the commonality of black Americans in genetic terms? Trower advised he reword that passage to avoid genetics, which would probably be a good idea. Surely Alvarez meant no harm, but one can quickly see why these stories are usually avoided in polite conversation. Here we can be thankful for an unusually frank discussion of race from a man whose ethnic identity we are trying to understand. It sets up Alvarez's social politics:

> It was a sobering experience, but it did not turn me into a social crusader—my life was to be devoted to physics; but my southern experience did make me appreciate the efforts of those who were just beginning to make their voices heard—crying out to right the wrongs of two centuries.[88]

The experience clearly made an impression on Alvarez, even if it did not make him a "social crusader." His political identity was formed at least in part on this trip in 1931. Reading only the public record, including his autobiography and accounts of his personality by others, one would be left guessing at his politics, but would probably

[86] *ibid*., p. 105.
[87] *ibid*., p. 104.
[88] *ibid*., p. 105.

assume that he was politically conservative. However, these few, edited-out pages give us a better understanding of who he was and how he saw race. Perhaps he declined those biographers not because he did not care about minorities, but because he genuinely did not think he could help. He believed that trying to present him as a minority would be "phony" and that young students would see right through it, even if his more public stance on nuclear weapons research gave the impression that he was a right-winger.

ALVAREZ IN MEXICO

At the 1932 American Physical Society national conference in Chicago, Alvarez met Manuel Sandoval Vallarta, a physicist at the Universidad Nacional Autónoma de Mexico who had been working at MIT.[89] Alvarez was a first year graduate student looking for a research project. Vallarta suggested a way to study cosmic rays that would best work at the altitude and latitude of his hometown, Mexico City. As Alvarez remembered, "Vallarta said he would take leave from MIT, and be our host in Mexico City."[90] Alvarez went to Mexico City for a month to perform cosmic ray experiments with some directional Geiger counters he had built. More than forty years later, Alvarez thanked Vallarta in a letter: "You made me feel so welcome in a country where, for the first time in my life, I did not speak the language."[91] In this revealing letter, Alvarez described his wish that he had learned Spanish, given the embarrassment of being named

[89] Vallarta was from the Universidad Nacional Autónoma de Mexico, but had an appointment at MIT at the time he published his *Outline of the Theory of the Allowed Cone of Cosmic Radiation* (Toronto: University of Toronto Press, 1938).
[90] Alvarez, Faculty Lecture, "Adventures in Nuclear Physics," University of California, Lawrence Radiation Laboratory, UCRL-10476 (Mar. 1962), p. 8.
[91] Alvarez to M. S. Vallarta, 8 Feb. 1974. LWAP, box 20, folder 4, "U-V 71-74."

Alvarez but not speaking any Spanish. This letter to Vallarta was a rare example of Alvarez wanting to enhance his identification with Mexico. However, he continued to call Vallarta's wife "Mary Luisa" while Vallarta called her "María Luisa."[92]

In addition to the language and cultural buffer Vallarta offered Alvarez, he also became something of a mentor. In the same letter, Alvarez told Vallarta: "You were a great source of strength to me in that period, and there is no way that I can thank you adequately."[93] Apparently, Vallarta was known for his supportive nature. Mexican engineer Alejandro Medina agreed.[94] After having worked with Vallarta, Medina described him as very supportive and friendly. Alvarez was one of the international scientists who corresponded with Vallarta, calling him by his first name, Manuel. Some even called him Manolo, a nickname for Manuel that implies familiarity. Medina adds, "There are no physicists in Mexico who do not owe him something and there are no young physicists who don't owe him nearly everything."[95] Medina was obviously impressed with Vallarta, as was Alvarez. One thing that Alvarez failed to mention was Vallarta's views on nuclear energy. This mentor held strong views against military uses of nuclear power,[96] although the hawkish Alvarez did not share these views.

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[92] *ibid*.
[93] *ibid*.
[94] Victor Alba, *Mexicanos Para la Historia: Doce Figuras Contemporáneas*. (Biblioteca Mínima Mexicana, 1955), p. 116. Medina does not mention Alvarez, but he said Vallarta had many informal correspondences with international scientists. Medina was an engineer who worked with Vallarta; Alba interviewed him for the chapter on Vallarta.
[95] *ibid*., p. 110. Translation by author.
[96] *ibid*., p. 116.

Unlike his father's time in the small Mexican mining town of Cananea, Luis's month in Mexico City "was extraordinarily pleasant."[97] In Mexico, Alvarez met "beautiful girls my own age who had been educated in the United States and spoke perfect English but who were undatably well chaperoned."[98] Meeting English speaking women must have been a comfort, since Alvarez did not know Spanish and was still single. Like his annoyance at his parents for not teaching him Spanish, Alvarez sometimes regretted not knowing more about his little bit of Hispanic-ness.

ALVAREZ THE MINORITY

Alvarez was right about not being a disadvantaged minority. His only brush with discrimination was one he told often and in good fun. After working on radar during the Second World War, he went off to Los Alamos to work on the detonator for the atomic bomb: "The state of interracial relations at Los Alamos at that time is best illustrated by the following story, which some people have thought was apocryphal, but was really true."[99] He recalled that he "greatly enjoyed working there, meeting old friends, and making new ones,"[100] as if the story had been told so often and to such effect that people thought it was a joke. Ultimately, it would be a funny story for Alvarez, and not a case of discrimination.

Alvarez and his new wife, Geraldine Smithwick, moved to Los Alamos so he could join the atomic bomb effort. The couple already had one son, their toddler Walter,

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97 Alvarez, *Adventures*, p. 27.
98 *ibid*.
99 Alvarez, LWAP undated autobiography draft carton 3, folder 14 "Misc. Autobiography draft 4," p. 886.
100 Alvarez, *Adventures*, p. 129.

and their stay at Los Alamos produced their second child, Jean, so having enough space for a new family was a priority. "Housing was a big issue at Los Alamos, and the early, high status people had the houses that had been built for the faculty of the Los Alamos Boys' School."[101] Those houses had bathtubs—they were known as "Bathtub Row"—whereas all of the apartments built by the Army had only showers. Having arrived later due his radar work, Alvarez and family would not get into Bathtub Row, but instead were assigned to one of the four-unit apartments, two upstairs and two downstairs. Alvarez reported that in the winter, the furnace ran "full blast," "stoked by the Mexican-Americans who did most of the dirty work on the hill."[102] The published version of Alvarez's autobiography described the crews as "Chicano furnace crews."[103] The clear division of labor was produced partly by the influx of highly skilled physicists, but also by the ready supply of cheap labor:

> Some of our Army men were technically skilled; others served us as laborers. A large force of Spanish-American women imported from nearby towns loaded detonators behind armored shields. We used insensitive explosives and lost not so much as a finger.[104]

The labor was often both gendered and racialized, but at least the menial work was not harmful.

Alvarez was a friend of physicist Johnnie Williams from his time at the University of Chicago and Johnnie's wife, Vera, ran the housing office, so she arranged

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101 Alvarez, LWAP undated autobiography draft carton 3, folder 14 "Misc. Autobiography draft 4," p. 886.
102 *ibid*.
103 Alvarez, *Adventures*, p. 129.
104 *ibid*., p. 135.

to get them the first good apartment that came available. Unfortunately, this created a problem for their new neighbors:

> When three other wives in the building learned that they were to have a neighbor named Alvarez, they rushed to Vera to complain of their imminent loss of social standing—they certainly didn't count Spanish-Americans among their best friends.[105]

In an early draft of Alvarez's autobiography, the reason for their distress was "their close proximity to a Mexican-American family."[106] Alvarez was not aware of the trouble until Vera "later told"[107] him, but apparently, "Spanish-Americans" were for furnace stoking, not socializing. It may be helpful to note that many Hispanics in New Mexico identify as "Spanish-American" because their Spanish ancestors settled when it was New Spain, before Mexico declared independence. Alvarez may have merely meant local Hispanics like the furnace crews or the detonator crews. Alvarez's graduate student, Lawrence Johnson, remembered the event slightly differently:

> At the next town meeting, the wife of a well-known European physicist took the floor to protest a Hispanic-American family moving into her apartment building. She must have been surprised when she found out who this family was.[108]

In this version of the story, the complaint was very public, as if everybody would understand the indignity. It is not clear what happened next, except that "Vera calmed their fears,"[109] presumably letting this "wife of a well-known European physicist" know

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105 *ibid*., p. 129.
106 Alvarez, LWAP undated autobiography draft carton 3, folder 14 "Misc. Autobiography draft 4," p. 887.
107 *ibid*.
108 Lawrence Johnston, "The War Years," in W. Peter Trower, ed., *Discovering Alvarez: Selected Works of Luis W. Alvarez, with Commentary by His Students and Colleagues*. (Chicago: University of Chicago Press, 1987), p. 63.
109 Alvarez, *Adventures*, p. 129.

that Luis Alvarez was, in fact, very white. The issue was settled, and if anything, Alvarez was more of an insider than the complaining wife, counting as a good friend the head of the housing office. However, it is not entirely clear that Vera did not just inform the complaining wives of Alvarez's status among physicists. After all, Alvarez headed the group that worked on the detonators for the implosion design. Richard Feynman describes his time at Los Alamos in a chapter titled "Los Alamos from below," but Alvarez was not "flittering about underneath," he was "one of the people in the higher echelons."[110] Alvarez was cleared of any Spanish association.

As far as the sources are willing to cooperate, this seems to be an isolated case where Alvarez came face to face with his ethnicity as one-quarter Spanish-American. Either Alvarez was seen as Spanish-American but socially accepted for his accomplishments, or he was able to convince the complainers that he was as white as they were. He certainly looked as white, but Spanish-Americans were explicitly not accepted although the Spanish *are* white. On the other hand, it should seem obvious that he was a prominent scientist if he worked at Los Alamos as a physicist. In 1963, Alvarez mentioned in a letter to the president of the American Physical Society that he had "never heard of a Spanish physicist."[111] Since most of the people he worked with at Los Alamos were physicists—even engineering jobs were often done by physicists—and the fact that

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[110] Richard Feynman, *"Surely You're Joking, Mr. Feynman!": Adventures of a Curious Character* (New York: W. W. Norton, 1985), p. 90.

[111] Alvarez to Karl K. Darrow, President, American Physical Society, 27 Mar. 1963, LWAP, box 2, folder "D 1956-64." Darrow had forwarded a letter to Alvarez from an Antonio Sánchez Alvarez of the Spanish organization Centro Español de Estudios Físicos. Darrow thought it "is probably to be regarded as something amusing," due to the poor translation into English. (Darrow to Alvarez, 19 Mar. 1963, same folder). Alvarez replied that he had never heard of them.

Spain was recovering from a civil war and not allied with the United States, it seems highly unlikely that there were any Spaniards at Los Alamos. Alvarez reported that at Los Alamos, "no one ever said that 'Some of my best friends are "Spanish-Americans."''"[112] There were certainly some Italians such as Enrico Fermi and Emilio Segrè. Being southern European did not seem to be the problem. Most likely, the concern was that Alvarez was a Mexican furnace-stoker. "Spanish-American" and "Hispanic-American" were simply euphemisms for Mexican. Either way, he passed this test with flying colors—his peers accepted him as prestigious and white. That makes this a good story for him to tell his friends.

POLITICS AND WHITENESS

Not long after World War II, Alvarez developed a political reputation compatible with his whiteness. He lived and worked at Berkeley through the sixties, the home of the Free Speech Movement and much political upheaval.[113] The physics department was polarized along political lines as Ernest Lawrence and Robert Oppenheimer split over nuclear weapons research. Alvarez was a Democrat, but a very firm hawk on nuclear weapons. He said, "Jack Kennedy was very much a hero of mine,"[114] and that president, like Alvarez, was a social liberal but a hardliner against the Soviet Union. While many physicists were disturbed by their role in developing the atomic bomb during the

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[112] Alvarez autobiography draft dated 1987, LWAP, carton 3, folder 10 "Draft of 'Adventures of a Physicist," p. 514.

[113] For more on politics at Berkeley, see W. J. Rorabaugh, *Berkeley at War* (New York: Oxford University Press, 1989). Alvarez's associate at Berkeley, Glenn Seaborg, became chancellor of the university from 1958 to 1961, Glenn Seaborg, *Chancellor At Berkeley* (Berkeley: Institute of Governmental Studies Press, University of California, 1994).

[114] Alvarez, *Adventures*, p. 222.

Manhattan Project, Alvarez championed the development of a thermonuclear bomb, also known as a hydrogen bomb, or "the super" as physicists called it. His position on the political issues relevant to nuclear physics often put him at odds with his peers and fed into an identity of whiteness.

Alvarez generally avoided very public activism in politics, but the dictation for his autobiography was free of the type of self-censorship and decorum that one finds in the final book. Dictating on 5 June 1972, Alvarez described his political progression from a Republican to a Democrat:

> Starting from my parents' Republican leanings, I moved gradually toward the Democratic position, and after many years of never having registered to vote in the California primary elections, I finally decided that since I really thought like a Democrat, I should join the party.[115]

However, he had a policy not to sign any political petitions:

> Many of my friends have taken an active part in politics, but I have refrained from doing this, in the feeling that the competence I know I have in the physical sciences is not convertible at some standard exchange rate into expertise in the political area. For this reason, I have refrained from signing the hundreds of petitions that have been presented to me, with one conspicuous exception.[116]

That exception was a "Scientists and Engineers for Johnson" petition that Jerome Weisner, President Kennedy's science advisor, had asked Alvarez to sign in 1964 in opposition to Barry Goldwater's aggressive stance on Vietnam. He later regretted that signature after President Johnson expanded the war and the journal *Science* wrote

> that there were a lot of scientists who wanted to forget that they had been members of "Scientists and Engineers for Johnson," and in a conspicuously

[115] Alvarez autobiography draft dated 5 Jun 1972, LWAP box 46, folder "Autobiography p. 101-200," p. 105.
[116] *ibid*., p. 106.

> outlined box, the magazine published the names of the founding members, with my name listed first in alphabetical order.[117]

That seemed to be lesson enough for Alvarez, so upon winning the Nobel Prize in 1968, "I promised myself that I wouldn't become a 'professional' laureate."[118] In social situations, he saw himself as "someone who tries to avoid personal controversy."[119] He provided an anecdote that justified his stance. Physicist Robert Millikan once explained his stance on the economy to the Berkeley student paper. When asked if he would like to comment on Millikan's policies, economist Ira Cross replied, "I wouldn't like to do that, but I'd be very happy to tell you my theories on the origin of Cosmic Rays."[120] However, Alvarez's reluctance to take public positions did not stop him from getting involved in two closely related controversies: "the super" and the investigation of Robert Oppenheimer.

Alvarez was enthusiastic about efforts to develop nuclear weapons in hope that they would be a deterrent to future wars. In the letter he wrote to his son, Walt, while flying back from Hiroshima, Alvarez hoped "that this terrible weapon we have created may bring the countries of the world together and prevent further wars."[121] Others described his position differently. David Lilienthal, head of the Atomic Energy Commission, referred to Edward Teller, Ernest Lawrence, and Luis Alvarez as "a group of scientists who can only be described as drooling with the prospect [of an H-bomb

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117 *ibid*.
118 Alvarez, *Adventures*, p. 276.
119 *ibid*.
120 Alvarez autobiography draft dated 5 Jun 1972, LWAP box 46, folder "Autobiography p. 101-200," p. 107.
121 Alvarez, *Adventures*, p. 8.

program] and bloodthirsty."[122] The Berkeley physics department was divided over this. Ernest Lawrence was Alvarez's mentor and he was a hawk. Robert Oppenheimer and others were against expanding beyond the Nagasaki-type bomb that they believed was powerful enough.

Alvarez may have regretted his dedication to that cause. He spent a great deal of time in the early 1950s working on the Materials Testing Accelerator (MTA) designed to produce enough neutrons to convert thorium to fissionable uranium 233 and to convert hydrogen into the tritium needed for a thermonuclear bomb.[123] Two years of Alvarez's career went into that military project, precluding him from doing any science. Alvarez wrote, "It was not a happy time."[124] He did not keep up with particle physics and his "output of physics papers dropped way off in this period."[125] Upon returning after two years with the MTA, he found himself socially distanced from his peers.

> Back at Berkeley but no longer an active player, I contrived a series of rationalizations to avoid having lunch with those who were. I had always kept up my end of physics conversations; now that I wasn't active I had little to contribute and didn't completely understand what I heard. It was a new experience. It was also uncomfortable. I found myself lunching in the company of the scanners, technicians who examined nuclear emulsions. They were friendly, attractive young people, the conversation was light, and I felt diverted. It never occurred to me that I was really hiding out from my physics friends, afraid of exposing my ignorance.[126]

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[122] A. Constandina Titus, *Bombs in the Backyard: Atomic Testing and American Politics*. (Reno: University of Nevada Press, 1986), p. 30, citing David Lilienthal, *The Journals of David E. Lilienthal, Vol. 2: The Atomic Energy Years, 1945-1950* (New York: Harper & Row, 1964), p. 582.
[123] Note that U-235 is the usual uranium isotope used in atomic weapons, but U-233 is also fissionable.
[124] Alvarez, *Adventures*, p. 173.
[125] *ibid*., p. 177.
[126] *ibid*., p. 176.

Alvarez had often noted the pull to become an administrator, but he had allowed his devotion to Lawrence to take him out of the physicist community at Berkeley.

In 1954, Alvarez testified against Robert Oppenheimer when the latter was accused of being a security risk and having Communist connections. Oppenheimer had been a socialist in the 1930s before the Soviet Union was an American adversary, but during the McCarthy years, any such connection brought a great deal of scrutiny. Many scientists testified in support of Oppenheimer; many others refused to testify against him. Ernest Lawrence had clashed bitterly with Oppenheimer on the super and on politics in general, but he called Alvarez "and announced emotionally that he wouldn't testify and that I shouldn't either."[127] Lawrence told Alvarez "that he, Ken Pitzer, Wendell Latimer, and I were viewed as a cabal bent on destroying Robert."[128] A call from Atomic Energy Commission chairman Lewis Strauss convinced Alvarez to testify. Although Alvarez's testimony was largely ineffectual, the decision to testify against the wishes of his mentor and many of his peers was a powerful statement. Alvarez argued that Lawrence's relationship to Oppenheimer is misunderstood. He argued that they disagreed on politics and culture, but that they remained "close personal friends. Ernest's second son was named for Robert Oppenheimer," but that "Ernest came from a small town in South Dakota; Robert was brought up in a wealthy and cultured home in New York City."[129] Alvarez did not attempt to get Oppenheimer in trouble, but he felt a duty to his country to

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[127] *ibid*., p. 180.
[128] *ibid*.
[129] *ibid*., p. 77

testify, despite Lawrence's wishes. Oppenheimer, possibly the man most responsible for developing the American atomic bomb, lost his security clearance.

Among physicists, the fact that Alvarez testified at the Oppenheimer hearings was not forgotten; he made many enemies. Astronomy professor George Greenstein got a clear impression from his colleagues:

> "Luis Alvarez, wasn't he that son of a bitch out at Berkeley?" inquired a colleague when I mentioned his name not long ago.
>
> "Was he a son of a bitch?" I responded.
>
> "Well, I never knew him personally, but I understand that he was."[130]

It is striking to hear such a denunciation in a journal. One physicist at the Oppenheimer hearings, Dr. Jerrold R. Zacharias, testified regarding Alvarez: "I think it is his arrogance … that bothers me the most."[131] Alvarez's testifying against Oppenheimer earned him a right-wing reputation.

Alvarez knew about his reputation. He had his critics, but he also had a loyal following that helped shield him from criticism. In his autobiography draft, he wrote,

> No doubt, some of my younger readers have already written me off as a very callous person who had no sympathy for all the people of this period who were subject to social injustices. I can't agree with this assessment; I am quite confident that had they lived … [132]

He cut himself off there as he regrouped. He went on to say that it was the Roosevelt "administration that was the first in this country to make it 'respectable' to be a union

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[130] George Greenstein, "Luie's Gadgets: A Profile of Luis Alvarez," *American Scholar* 61, no. 1 (1992), pp. 94-95.
[131] US Atomic Energy Commission, *In the Matter of Robert Oppenheimer*. (Cambridge: The MIT Press, 1971), p. 932. The MIT version is preferable to the 1954 Government Printing Office version, since it includes an index.
[132] Alvarez autobiography draft dated 5 Jun 1972, LWAP box 46, folder "Autobiography p. 1-100," p. 100.

sympathizer or to admit that there was a lot of social injustice in our country."[133] His first editor, former student Peter Trower, wrote "NONSENSE!" over that section. Alvarez made a few changes by hand, then Trower scribbled the whole section out and wrote "OMIT" in large letters. Trower could not imagine that young physicists would think Alvarez was "a very callous person." He encouraged Alvarez to publish his autobiography and arranged for a festschrift for his mentor, *Discovering Alvarez*.[134]

The community of physicists knew about Alvarez's testimony against a fellow physicist and since he did not sign petitions, he would be known only for his work on the super and against Oppenheimer, actions that made him look conservative. While even a poor son of Mexican immigrants can be politically conservative, this choice generally has an impact on one's ethnic persona. Mexican-American writer Richard Rodriguez said that his opposition to affirmative action led to him being called "a 'coconut'—someone brown on the outside, white on the inside."[135] Of course, Alvarez would reply that he was not "brown on the outside." Despite being a Democrat, he became mostly known among physicists for the conservative politics of anti-communism.

RACE IN THE LAB

One wonders what exactly race has to do with science. Alvarez's life seems to be full of anecdotes about his whiteness or people's assumptions about his Hispanic-ness.

133 Alvarez autobiography draft dated 5 Jun 1972, LWAP box 46, folder "Autobiography p. 101-200," p. 101.
134 Peter Trower, ed., *Discovering Alvarez: Selected Works of Luis W. Alvarez with Commentary by His Students and Colleagues* (Chicago: University of Chicago Press, 1987).
135 Richard Rodriguez, *Hunger of Memory: The Education of Richard Rodriguez* (New York: Bantam Books, 1982), p. 162.

However, scientific papers are devoid of ethnic remarks or racial analysis. The only way to learn about race in the lab is to go beyond the sanitized, prepared literature of scientific journals. The dirt is in the lab notes.

Sorting through seven cartons of Alvarez's Berkeley lab notes covering 14 March 1956 to 17 May 1969, one can begin to guess at the everyday life in the lab. The lab was workplace for graduate students and technicians. Professors and heads of groups like Alvarez came to the lab only sporadically, instead administering from afar. However, the group's character was often defined by the group leader, possibly because the graduate students had to be prepared for a visit from their professor. That said, one can draw his or her own conclusions about limericks and drawings scattered through the notes. One can guess about the significance of a Star of David next to the date for Hanukah. One can wonder about party flyers and comments about getting women into the lab, and wonder how they aimed the chamber camera at a woman's breasts. For practical purposes, I will concentrate on slightly more concrete evidence.[136]

Each lab entry is dated and signed. Despite the historian's struggles with handwritten archival material, much can be learned about who worked on Alvarez's equipment. Further, it becomes apparent that there are a number of Spanish-surnamed people. Names like Ron Rínta, F. Barrera, and chief engineer Paul Hernandez were sprinkled though a time sheet of sixty-nine names.[137] In his autobiography, Alvarez only

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[136] Cartons 1-7 of the Luis W. Alvarez bubble chamber logs, BANC MSS 76/161c, The Bancroft Library, University of California, Berkeley (LWABCL), are a collection distinct from the seventy-eight box and three carton collection and include lab notes for Alvarez's work at Berkeley. I will only footnote important finds, not lists of where to find nude photographs or party announcements.

[137] LWABCL, carton 2, lab notebook: "15" Bubble Chamber log 5," dated 1959, March to April.

mentioned Hernandez once, crediting him for the lab's safety record: "[Hernandez] took even our most outlandish proposals seriously and applied all his ingenuity to making them safe before giving up those that proved irremediably flawed."[138] Since they worked with liquid hydrogen, safety was a major concern that engineers took more seriously than the sometimes cavalier physicists. Historian Peter Galison described the 1965 explosion that killed one man at Harvard's Cambridge Electron Accelerator.[139] Galison argues that the explosion was a major blow to the Harvard approach to particle physics and left the Berkeley method dominant. However, even if Hernandez is to be credited with safety at Berkeley, engineers are not as celebrated as physicists. One is left to wonder if three Spanish surnames out of sixty nine was a lot in 1959 Berkeley or not. Historian W. J. Rorabaugh provided some context: in 1960, the city of Berkeley was 74% white, 20% black, 6% Asian, and less than one percent "other."[140] One wonders if Alvarez sought out or avoided Hispanic technicians, or if they were attracted by his surname. Most likely he did not use ethnicity to make employment decisions. One also wonders if their last names are as misleading as Alvarez's. I have not been able to determine how Hernandez self-identified.

Another perspective emerges in the political drawings and flyers found in the pages of lab notes. In the lab notebook covering December 1968 to May 1969, one of the students left a flyer announcing a rally. "Don't let Heynes fence us out," it declares,

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138 Alvarez, *Adventures*, p. 190.
139 Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: The University of Chicago Press, 1997), p. 353.
140 Rorabaugh, *Berkeley at War*, p. 173. It is also worth noting that in 1964, the Lawrence Berkeley Laboratory entailed 20% of UC Berkeley's Full-Time Equivalent employment.

describing the police in riot gear and snipers atop buildings to prevent a takeover of the plot of land now known as People's Park.[141] On the other end of the political spectrum, there is an elaborately hand drawn sign that says, "Fight Poverty—Shoot a Pauper!"[142] It is difficult to ascertain what is included as a statement and what is included out of incredulous curiosity. One photocopied, fictional tire advertisement clarifies the issue. The handwritten ad for Italian snow tires claims that "Dago thru snow, dago thru water, dago thru mud, dago thru ice, and when dago flat, dago wop! wop! wop!"[143] Whoever included these flyers must have known that Alvarez would collect and read the lab notebooks, but chances are that Emilio Segré, the Italian scientist working down the hall, would never see these notes. No one would have left these flyers in the lab notes if he thought Alvarez was anything like that Italian immigrant.

Analyzing race in the scientific community may reveal a little about why Alvarez chose not to take on a Spanish or Hispanic identity. While learning Spanish could not have been very difficult, one must ask, how could it have helped? German and French were helpful for reading physics journals. Claudio Segrè recalled that his father and other immigrants at Los Alamos—General Leslie Groves's "crackpot foreign" scientists—could avoid security regulations by speaking their native languages.[144] A physicist fluent in German or Italian could avoid the security at Los Alamos or at many physics labs around the country, but it could also help socialize a physicist into a clique,

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141 LWABCL, carton 7, lab notebook: "25" Bubble Chamber log 13," dated 18 December 1968 to 17 May 1969.
142 LWABCL, carton 7, lab notebook: "25" Bubble Chamber log 15," dated 8 October 1969 to 1 March 1970.
143 *ibid*.
144 Emilio Segrè, *Atoms*, pp. 37, 40.

an instant social group awarded by nationality. Spanish would have been of little use except for talking to the maintenance crew.

CHANGING PUBLIC IMAGE

Alvarez was careful about his public image and firm about his politics. Filmmaker Peter Batty's request to include Alvarez in a documentary film on the hydrogen bomb received a curt "Thank you – no." Alvarez explained that

> I would prefer not to participate since I find that my views on such matters differ from the conventional ones. … There is no way that I can change people's deeply felt sentiments about the bomb, in a few minute interview before a camera, and experience tells me that if I express such views in public, people for the most part simply regard me as a beastly fellow.[145]

Alvarez's nuclear politics were clear, and for that reason, he sought to control the type of writing about him whenever possible.

In 2006, Raintree published Tina Randall's *Luis Walter Alvarez* as part of its Hispanic-American Biographies series.[146] Now that Alvarez is no longer able to correct misguided biographers, having passed away in 1988, his image will likely drift toward becoming seen as one of the greatest Hispanic scientists alongside Mexicans, Cubans, and Argentineans. While living, he had some ability to encourage writers to tell his story without using race and to criticize others who insisted on analyzing his ethnicity. With his death, a new generation of biographies has appeared of which he clearly would have disapproved. "Luie"[147]—as his friends called him—is now the Luis Alvarez of *Hispanic*

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[145] Alvarez to Peter Batty 7 April 1974. LWAP, box 17, folder 1, "B 1971-74."
[146] Tina Randall, *Luis Walter Alvarez* (Chicago: Raintree, 2006).
[147] See, for example, Alvarez's correspondences with Bob Shankland, LWAP, box 20, folders 1 and 2.

magazine's "Latinos in the Lab."[148] That article's subtitle seems to exclude Alvarez from the start: "How Scientists Have Overcome the Odds to Become Leaders and Mentors." Alvarez did not overcome the odds—the implication is that Latinos come from poor, uneducated families—and he was not a mentor to Latino youth. Himilce Novas's *The Hispanic 100: A Ranking of the Latino Men and Women Who Have Most Influenced American Thought and Culture* ranked Alvarez third behind Cesar Chavez and Texas Representative Henry González despite including no mention of Alvarez's self-identification.[149] Presumably, the list measured people's impact on white American thought and culture if they included Alvarez. *The Hispanic-American Almanac* listed Alvarez, implicitly classifying him as Hispanic-American by inclusion under the heading "Prominent Hispanic Scientists," without having to explain what made him Hispanic.[150] Presumably, the name was enough and the short, one-paragraph biography leaves little space for that discussion. Within a few years after his death, including Alvarez in a list of Hispanic or Latino scientists was standard practice. He had no say in the matter any more.

If there is one sure way to ingrain an idea into the public subconscious, it is to teach it to children. Once a generation of children grows up believing something, it is fixed in their memory. Corinn Codye wrote a children's book on Luis Alvarez in 1990 for the Raintree Hispanic Stories series. This thirty-two page picture book told a

[148] Dan DeVries and Melanie Cole, "Latinos In the Lab: How Scientists have overcome the odds to become leaders and mentors," *Hispanic Magazine* 9 (1 Sep 1996), 36-42.
[149]Himilce Novas, *The Hispanic 100: A Ranking of the Latino Men and Women Who Have Most Influenced American Thought and Culture* (New York: Citadel, 1995).
[150] Nicolás Kanellos, *The Hispanic-American Almanac: A Reference Work on Hispanics in the United States* (Detroit: Gale Research Inc., 1993), pp. 677-678.

surprisingly complete account of Alvarez's career. Yet it is ironic that this children's book about a man who could not read Spanish was written with English and Spanish on each page. The Raintree series was loose with its definition of Hispanic. Another volume in the series on the physician Carlos Finlay noted that his parents were from Sweden and France, but Carlos was Hispanic because he grew up in Cuba.[151] Apparently, either a Spanish surname or exposure to a Latin American culture made one Hispanic. General Consulting Editor Frank de Varona added, "All of these contributions [of Hispanics] were a part of the development of the United States and its rich and varied cultural heritage."[152] No doubt America has a rich and varied cultural heritage. As Alvarez pointed out to the Carnation Company, "I happen to have four grandparents, one born in Ireland, one in Germany, one in Spain, and one in this country."[153] He could easily have chosen any of these identities, but he opted for a middle ground of sorts. Instead of accentuating each of his grandparents' lineages, he largely allowed them all to fade away; that is, to fade to white.

In addition to biographers' interest in Alvarez, Latino physicists were anxious to hear how Alvarez could help minorities in science. In 1968, Andrés Sanchez, a former Berkeley physics undergraduate and technician at the Arecibo telescope in Puerto Rico, wrote to Alvarez. After outlining his plans to combine science, education, and minority issues, Sanchez asked Alvarez for information or advice. Alvarez replied that he was

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[151] Christine Sumption and Kathleen Thompson, *Carlos Finlay*. (Milwaukee: Raintree Publishers, 1990).
[152] Corinn Codye, *Luis W. Alvarez*. (Milwaukee: Raintree Publishers, 1990), inner cover, "note to the reader."
[153] Alvarez to Ms. Betty Logas, Carnation Company, 12 September 1972. LWAP, box 17, folder 4, "C 1971-74 [2 of 2]."

interested in the issues but had little to contribute. Instead, he recommended that Sanchez contact the university administration, basically avoiding the question. Surely, Sanchez would not ask Alvarez for more help, but others would have to learn on their own.[154]

Orlando Aguero wrote to Alvarez in 1957 asking for help in translating a dictionary of electrical terms into Spanish. Evidently, he did not know much about Alvarez and certainly did not know that Alvarez did not speak Spanish. Alvarez politely replied that he could not help and instead recommended that Aguero write to Manuel Sandoval Vallarta.[155] Like some of Alvarez's biographers, Aguero must have simply looked for the most prominent scientist with a Spanish surname. Again, like the biographers, he assumed Alvarez was Latino and a Spanish speaker, while never having heard of Vallarta. The "Latinos in the Lab" collections never have anybody else as respected and accomplished as Alvarez.

David E. Newton's *Latinos in Science, Math, and Professions* continued the practice of listing Alvarez as a prominent Latino scientist in 2007.[156] This compilation differed in that it went to greater lengths to define and justify the Latino scientist listing. Nearly the entire "Author's Note" that explains the methodology is worth quoting because it gets at what is at issue when one includes Alvarez in a compilation like this.

> The book uses the word *Latino* in its title to describe these individuals, although a number of comparable terms have also been used for these men and women. One sometimes sees the terms *Chicano* and *Chicana*, *Hispanic*,

[154] Andrés D. Sanchez to Alvarez, undated, and reply of 5 July 1968. LWAP, box 1, folder 4, "S 1965-68 [2 of 2]."
[155] Orlando Aguero to Alvarez, 27 February 1957 and reply, 12 March 1957. LWAP, box 1, folder 1, "A 1956-64."
[156] David E. Newton, *Latinos In Science, Math, and Professions: A to Z of Latino Amerians* (New York: Infobase Publishing, 2007).

> *Iberian*, *Mexican American*, *Cuban American*, or *Dominican American*, for example. Each term is more or less different from the others, a difference that sometimes has considerable significance to individuals to whom it may (or may not) apply.[157]

Alvarez would argue that he was none of those, but he would agree that his racial identity had "considerable significance." Newton acknowledged the issues surrounding ethnic definitions:

> For example, some people argue that the term *Hispanic* refers only to those individuals whose culture can be traced directly to Spain itself. Similarly, the terms *Latino* and *Latina* are sometimes reserved for individuals whose roots are in Latin America and not specifically in Spain. Although differences in terminology may seem to be insignificant points of semantics, they may be far more than that to some people. In fact, some people contacted for inclusion in this book declined to be listed because they did not define themselves as *Latino* although they were, in fact, Spanish-speaking or from a Latin American country or met the author's criteria for inclusion on some other basis.[158]

Alvarez was not excluded. He almost certainly would have "declined to be listed": the trail of correspondence with would-be biographers is clear. However, a fluid enough definition of Latino could surely be devised that would allow Newton to include Alvarez. Newton laid out his definition:

> To be clear, the basis for inclusion in this listing of important Latinos in the sciences is that an individual (1) come himself or herself from a Spanish-speaking country, such as Spain, Mexico, or Cuba, or have an ancestry that can be traced to such a country; (2) have lived for some significant period of time in the United States; and (3) have achieved some prominence in one of the physical sciences, mathematics, social sciences, or in the field of invention.[159]

Alvarez was not from a Spanish-speaking country, but he did have ancestry from Spain. He definitely lived in the United States and he definitely achieved prominence in the

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[157] Newton, *Latinos*, "Author's Note," p. *xi*.
[158] *ibid*.
[159] *ibid*.

physical sciences. So according to Newton, he qualified for inclusion. Newton's volume points out that Alvarez's grandfather was from Spain, but also says that "Luis Alvarez's mother was Harriet Skidmore Smythe, whose family was of Irish origin."[160] Nobody said that a Latino could not also have other ethnicities. Certainly a Chicano is allowed to be part Native American—it is almost required.

What of Alvarez's claim that he was not a disadvantaged minority? This is the justification for promoting the success of ethnic and racial minorities. Newton would likely not have worked on a "Whites in Science" book. Newton emphasized this significance of achievement among minorities:

> While it is true that some men and women came from well-to-do families in which a college education and professional success were taken for granted, a remarkable number of the scientists described here struggled to overcome prejudice, poverty, lack of English-language skills, and other deficits that would probably have deterred many of their contemporaries, both Latino and Anglo.[161]

Obviously, Alvarez had none of these disadvantages. This was the implication that drove Alvarez to protest that, because he was not disadvantaged, presenting him as a minority was "phony." Newton allowed that some of his Latinos "came from well-to-do families," so the grandson of a wealthy Los Angeles landowner and son of a prominent medical doctor and author could still be included in *Latinos in Science*.

Newton described what could cause exclusion from his book: "Many outstanding Latino scientists have not been included for a number of reasons, including the author's inability to obtain a complete set of biographical data, the individual's unwillingness to

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[160] *ibid*., p. 8.
[161] *ibid*., p. *ix*.

be listed, or lack of space to include all qualified men and women."[162] Yet Alvarez remained in the book. Presumably, Newton did not know that Alvarez wanted to be excluded and since he had passed away in 1988, Alvarez would remain a part of this 2007 compilation of great Latinos.

A final twist happened in this story of Alvarez's shifting identity. In 2002, this author gave a talk at the History of Science Society on Alvarez's ethnicity. With the quick spread of information in this Internet era, the talk attracted some attention. I received an email from Luis Alvarez's son, Donald Alvarez, inquiring about the talk. A follow-up proved fruitless after he did not reply to further questions. A handful of others also contacted me. Gradually, biographies of Alvarez have deemphasized his ethnicity. In 2010, Mike Venezia's *Luis Alvarez: Wild Idea Man* included only this on his ethnicity: "He was of Spanish-American descent."[163] It is true that Alvarez had some Spanish descent. Venezia did not elaborate, and the book was not part of a Latinos in science series. It was a children's book that gives a simplified but honest appraisal of Alvarez's career with a new emphasis on his ability to come up with unconventional solutions to problems in physics, aviation, geology, and as an inventor. Perhaps Alvarez's public identity will shift again, but as of 2011, an Internet search for Luis Alvarez returns many Hispanic-themed websites.

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[162] *ibid*.

[163] Mike Venezia, *Luis Alvarez: Wild Idea Man* (New York: Children's Press, 2010).

A NEED FOR ROLE MODELS

At the Houston national conference for SACNAS,[164] the Society for the Advancement of Chicanos and Native Americans in Science, which this author attended in October 1997, University of Arizona physicist J. D. García expressed his blunt dislike for Alvarez based on politics and Alvarez's identity. Self-identifying as an Arizona Chicano from roughly the same generation as Alvarez, García confided that many Chicano physicists were disappointed by Alvarez's choice of ethnicity. García made a point of calling himself Chicano "for political reasons." It would seem that some people in a position similar to Alvarez made clear decisions to maintain a Hispanic identity, meaning that Alvarez probably could have done so as well. García elaborated that Alvarez was Hispanic whether he wanted to be or not; his choice of whiteness was "unfortunate." Alvarez never let on that he had a choice. Not only did García clarify that Alvarez chose his identity, but that many Chicano physicists opposed his choice.

García answered many questions about Alvarez's image. One final thing he cleared up was the need for conferences like SACNAS. García explained that he often felt isolated as a Chicano physicist. SACNAS was a great reminder of one's ethnicity. "It's nice to know you're not alone," he said. Although SACNAS' explicit aim was to encourage cooperation, García had never worked with another Chicano physicist. He did not feel the need to. Instead, he was satisfied knowing that physics is for Latinos. One student, Gabriela, said that her "guero" advisor suggested she not go if she did not give a

[164] For more on SACNAS, contact SACNAS, University of California, 1156 High Street, Santa Cruz, CA 95064, (408) 459-4272 or www.sacnas.org.

presentation. She said she had to bite her tongue because the first thing she thought was, "You asshole, you don't even understand." While many students came from schools with many Latinos in their department, some did not. Some professors are isolated and seek out more people like themselves. SACNAS is an excellent opportunity to do this on a fairly large scale. Instead of the practical issues of collaboration and research—which were also covered by the conference—SACNAS allowed itself to become a social realm that allows people to meet other Chicanos and think about what it means to be Latino, Hispanic, Chicano, or Native American. Many Chicano scientists are grateful for this very opportunity. In this same vein, Alvarez biographers thought they found in his success the opportunity to prove that physics is for Latinos. Unfortunately, using Alvarez's success in that role is "phony."

To understand why Alvarez makes such a good focus for a study of whiteness is in part an introspective study—it may say more about Chicanos than about Luis Alvarez. Is this inappropriate? Introspective work such as this is very appropriate for a study on ethnic identity. It might seem out of place in a work on the history of science, even one about identity. This is not to say that a white man cannot do a history of Latinas or the other way around, but simply that a history is informed by an understanding of who the author is and what the author brings to the discussion. In this case, an analysis of my own motivations and intents is useful to understand other interpretations of Alvarez's life and career. I was originally led to read *Alvarez: Adventures of a Physicist* after reading *American Genesis* by Thomas Hughes in an undergraduate history of science class. Hughes mentioned him once on the difference between the cultures of the University of

Chicago laboratory and the lab at Berkeley.[165] Scouring the bibliography for more of the type of writing that initially attracted me to the history of science led to an autobiography with an intriguing title: *Alvarez*. As if to clear up any doubt, the author's given name was spelled Luis. Only nine pages in, after the Prologue on the Hiroshima mission, line one of chapter one clears it up, as if it was the first thing Alvarez wanted us to know about him: "People have sometimes been surprised that a tall, ruddy, blond should bear the name Luis Alvarez."[166] For me, this raised more questions than it answered.

What happens when a physicist has a Spanish surname? Would a young Chicano physicist run across European scientists and their wives who object to living next to Hispanic-Americans? Can a person decide to be white? Most people do not get to make that decision; it is made for them. However, in some cases, the individual decides. In these cases, what are the consequences? What is to be learned? Does Alvarez make a case for the whiteness of science? The young Chicano reader would not be let down to hear that Ernest Rutherford was white; there was no rollercoaster to precede the denouement.

CONCLUSION AND LESSON

Alvarez was a white physicist. His conservative nuclear politics placed him on the hawkish side of the Cold War divide among physicists. While many of the Los Alamos physicists later worked against a nuclear arms race, Alvarez thought nuclear weapons were a powerful and necessary deterrent, as did Ernest Lawrence, Edward

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165 Thomas Hughes, *American Genesis: A Century of Invention and Technological Enthusiasm, 1870-1970* (New York: Penguin Books, 1989), p. 406.
166 Alvarez, *Adventures*, p. 9.

Teller, and John Wheeler. This was in contrast to his social politics, which, as a Democrat, were likely sympathetic to minority causes. Somehow being a nuclear hawk seems whiter than being anti-nukes, if only because none of the people in a position of power to push for thermonuclear bombs were disadvantaged minorities. Alvarez testified for the government in Robert Oppenheimer's McCarthy-era hearing. Many in the scientific community grew to dislike him, but he professed never to care about others' criticisms. However, the historian should care because it is the public perception of Alvarez that will largely determine his value as useable history. He is already under-studied. It remains to be seen if future authors will find new uses for his unique career.

Richard Rodriguez was right. A college education distances a minority from his minority status. Luis Alvarez was two generations removed from his already white Spanish roots—so removed that he was not Hispanic. He was even more distanced from his Spanish roots than Rodriguez, who might have only Native Mexican ancestors. Rodriguez's argument holds in this case since Alvarez was so very adamantly a non-minority. Alvarez as part of minority culture was "phony." However, the efforts of biographers to include Alvarez based only on his Spanish name demonstrate the need for Latino scientists and role models. In Novas's *The Hispanic 100*, Alvarez ranked only behind Cesar Chavez and Texas Representative Henry B. Gonzalez. Texas Board of Education member Joe Bernal said of Gonzalez, "He was one of us. His name was Gonzalez, and you couldn't shy away from that." *Austin American-Statesman* staff writer Dave Harmon continued, "And although Gonzalez didn't harp on his ethnicity, Bernal said he was a role model for many young Mexican-Americans."[167] Rodriguez ignored this need for role models.

Alvarez's interaction with biographers demonstrated the difference between whiteness, Hispanic-ness, and even blackness. The Irish were, in certain regions and at certain times, more oppressed than Mexicans, but Alvarez's Irish blood was never an issue, despite apologetic claims from a Carnation Company lawyer. It would seem that the family name is key here. Yet a Louis Smyth could not easily escape his minority identity if a quarter of his blood were black under the one drop rule.[168] No matter the name, he would be as black as his black grandparent in social and political terms. In fact, a Luis Alvarez with a Spanish paternal grandfather and a black maternal grandmother could not have escaped being black, while his Spanish ancestry would surely be all but forgotten. This hypothetical Alvarez would not have the choice the real Alvarez had, the choice that, as J. D. Garcia argues, was so poorly decided. A quarter black may attempt to "pass" for white; a quarter Spaniard simply asks that people call him "Luie."

After much discussion, it remains unclear what Luis Alvarez's whiteness *was*. His ethnicity as a Hispanic scientist or Latino in the Lab was defined by a collection of writers through his ethnic name. This proved to be misleading. That he had a Spanish grandfather did not, from Alvarez's perspective, affect his identity as a white American. Yet his surname made him wish he could speak Spanish and gave him a curiosity about

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[167] Dave Harmon, "Texans Lose Their Crusading Congressman, Henry B." *Austin American-Statesman*, 29 November 2000.
[168] While there is the possibility of light-skinned blacks "passing" for white, the one drop rule and Jim Crow laws effectively kept most blacks from choosing their racial category.

Mexico and Spain. Was this simply Anglo tourist mentality? Alvarez's whiteness was somewhat defined by two factors: he was not Hispanic and he was just a physicist. In short, he was not a Hispanic physicist. That made him white; it made him Luie; it made him Chips. Textbook definitions of Hispanic and white would not have satisfied Luis Alvarez. Sometimes they are too simple, sometimes they are too confusing. They do not satisfy the historian who needs to explore meaning descriptively, anecdotally. We should try not to impose a definition on someone who spent seventy-seven years trying to define his identity. More to the point: this is a history about contested definitions with no right answers; many of the players were confused. Alvarez's history is not about reading definitions out of a book.

Sadly, none of the Hispanic scientists included in the compilations of great Latinos in science can compare to Alvarez's successes in physics or even paleontology, a field he entered accidentally. One can only wonder how young Chicano physicists will take the news that someone they took to be the greatest Mexican-American physicist was not even Spanish-American, much less Chicano. Even if Alvarez had chosen to become Chicano, of all things, his young Chicano admirers would eventually realize that the "Swedish Spaniard" was so nominally Spanish as to be a fake. Would young, brown physicists identify with him if they knew he was a "tall, ruddy blonde" with at least three generations of privilege? Worse, what if they come to the cynical conclusion that his success somehow hinged on his ability to deny that there was any significance to his Spanish heritage? Clearly, Alvarez was right. Attempts to frame him as an oppressed minority are "phony." The historian must accept Alvarez's whiteness and be thankful for the well-documented case study of blurred racial categories. However, the activist, the educator, and the parent should see in the attempts to frame Alvarez as Hispanic the value and necessity of role models. The confusion over Alvarez's surname shows that while Alvarez could not be a minority role model, Latino scientists should contribute to their community's self-worth in ways that will inevitably help them settle their own place in society.

Chapter 7: *Conclusion*

Like the Mexican-American activists who became Chicanos in the 1960s, like the black American activists who found a new pride in the Black Power movement, American atomic physicists underwent a transformation of self-image that began in the 1930s. That transformation was slower than the realization of Chicano or black pride, but it was thorough. From a class of scientists largely ignored by the public and content to remain in the shadows emerged a group of leaders in international science with a bravado and swagger that came from intense public attention, nearly endless government funding, and most importantly, a new and enlarged sense of self-worth. In the 1940s and 1950s, American atomic physicists became not merely the premier scientists in America, not simply the best physicists the world, but this handful of researchers became the very model for all the sciences globally.

Luis Alvarez is the unrivaled exemplar of this transformation because, more than any other participant or observer, he was there. He may not have been the smartest of the atomic physicists or the most accomplished, but he had an uncanny knack for finding the right place to be—he had a nose for the hot field. He had close connections to some of the greatest figures of the previous generation of American atomic physicists: Ernest Lawrence and Arthur Holly Compton. He was at Los Alamos where the power of the new physics became exemplified by the atomic bomb. He was the only person to see the first three atomic explosions at Trinity, Hiroshima, and Nagasaki — and he was there for all three not because his expertise in implosion fusing was needed over Japan, but because, more than anyone else, he wanted to be where things were happening.

More than even Ernest Lawrence, who died in 1958 before Alvarez built his largest bubble chambers, he drove the growth of the much-discussed "Big Science." Alvarez didn't invent the bubble chamber; Don Glaser did. However, when Glaser wanted to make incrementally larger ones, Alvarez insisted that they instead scale up by orders of magnitude. Even Lawrence thought Alvarez's proposed bubble chambers were too big. Lawrence, the man perhaps most associated with Big Science, thought Alvarez was going too big! However, he trusted Alvarez and gave him the green light. Bigger, faster, more efficient—Alvarez's work mirrored Henry Ford's assembly line or Gordon Moore's law of exponentially faster computer chips. Alvarez brought in engineers, automated data collection, and introduced computer analysis, imparting a distinct American manufacturing style to a field that had previously operated like a custom shop. Alvarez is the clearest example of Big Science as an American invention. The media and the public often fixate on the size of the cyclotron, ignoring the equally important data collection performed by Alvarez's hydrogen bubble chambers. As thoroughly as Big Science has already been studied, few of the historians covering this trend have written about Alvarez, and none have grappled with his real importance in the story.

Alvarez wanted to be remembered. Well before it became common for physicists—certainly American physicists—to write autobiographies, the death of Ernest Lawrence in 1958 made Alvarez start thinking about his own place in history. He wrote a full draft an autobiography as an American physicist in 1972 when any autobiographies of physicists tended to relegate the physics as incidental to an otherwise interesting life. For Alvarez, the physics is what made his life an adventure, whether it was radar or atomic bomb work in the Second World War or in bringing high-energy physics to the field of paleontology. He self-described himself as a physicist and as an American while he introduced an American way of doing high-energy physics.

History had a practical value for Alvarez that revealed itself in his textbook draft. Textbooks are tools for the enculturation of young proto-physicists; in this light, Alvarez raised an objection to the way physics was presented in journals and textbooks. He thought the cleaned-up version of physics—reporting only the successful results and not the failures, dead-ends, and mistakes—could not teach young physicists how to do physics. That information was often communicated orally, but was available in that form only to graduate students lucky enough to work with the great men of science. Those who worked with Lawrence, Alvarez, Fermi, Segrè, and the like had access to a virtual library of what had been tried, what worked, and what had not. However, this practical, pedagogical need also fed into Alvarez's personality to create a secondary use for history. Alvarez was an early advocate of collecting and teaching the history of atomic physics. He saved Lawrence's two discarded cyclotrons, wrote a self-consciously 'historic' letter to his son Walt while flying back from the raid on Hiroshima, and wrote both a historically minded textbook and an autobiography, filling both with the myths, legends, and heroes of American physics.

His nose for the hot field was keen enough that it sometimes led him into other fields. The very concept of identity relates to self-ascription and ascription by others of a different social group. In Alvarez, we have in his work outside of high-energy physics an opportunity to watch his interaction with other fields. We can see him bristle at the methodology in forensics. We hear his surprise to discover that geology may be a "real field" after all. We find him struggling with a culturally entrenched paradigm in evolutionary biology—gradualism—that leaves him unprepared for the backlash against his impact theory. At each turn, we get the opportunity to see the cultural lines in the sciences drawn and redrawn.

If there were any ambiguity in describing the cultural differences between different sciences, Alvarez's Nutfile gives us a clear relief of insider scientists versus those completely outside the sciences and academia. While a physicist might not understand paleontology, she should recognize the science of dinosaur extinction played by the same rules as physics. The specifics of a universal scientific method are often debated, but in the Nutfile, we get a further and much clearer demarcation between Alvarez and the lay physics enthusiast. Here we see how obvious it can be when someone does not understand the rules of science. Further, we studied the curious case of J.C. Cooper, who managed to skirt that boundary despite later revealing himself as a paranoid conspiracy theorist and, in Alvarez's view, a nut.

Finally, Alvarez left us a relatively rare case of a white man having to actively choose his whiteness. That is, this American physicist could arguably have chosen another identity to complement his physicist identity. He could have chosen to be a Spanish-American physicist. He could possibly have maintained a Hispanic identity. He did sometimes long for a closer connection with his Spanish heritage that he never voiced about his Irish or German background. He had ample opportunity to make that choice, given the many letters he received from Hispanic groups. However, he forcefully chose whiteness, which he may have seen as the ethnicity most compatible and less distracting from his persona as an American physicist. He certainly associated Hispanic with disadvantaged. He likely associated Hispanic with less American and possibly with less of a physicist.

This work is about American atomic physicists as a social group developing a self-identity. Alvarez's sense of history was fundamentally about recruiting new physicists; education is academic enculturation. I do not argue that Alvarez caused this transformation, but that he was an excellent indicator of it. Alvarez was present at the center of this transformation. He helped take American physics to the very top of all sciences worldwide. His style of bigger and better bubble chambers made the Lawrence Berkeley Laboratory the world's premier center for science of any kind anywhere. His style helped influence the successes of American atomic physics, but he was not alone in making American physics king. For the historian, he is the ideal model for studying the creation of the unstoppable physicist.

THE RISE OF MOLECULAR BIOLOGY

Comic book writer Stan Lee created two superheroes in 1962—Peter Parker became Spider-man after being bit by an irradiated spider while an exposure to gamma rays gave the Incredible Hulk his superhuman strength. The phenomena of modern physics were mysterious and filled the public with wonder. These two superheroes became the subject of a pair of films in 2002 and 2003. Lee consulted on the script, but he reformulated the two into subjects of genetic engineering. What happened to radiation in science fiction? What prompted Lee to dump physics in favor of molecular biology—had radiation lost its mysterious powers or was it simply not as cool anymore? Had molecular biology become the new trendy field? Why should science have trendy fields? Is science not a logical, detached discipline?

Of course, science is a human endeavor, so social dynamics will influence the development of a theory or body of knowledge. Before 1943, molecular biologists did not know the function of DNA, but progress came quickly. That year, Oswald Avery, Colin MacLeod, and Maclyn McCarty showed that it carries genetic information. In 1953, James Watson, Francis Crick, Maurice Wilkins, Rosalind Franklin discovered its double helix structure, suggesting that genetic information could be carried by the series of nucleotides inside the double helix. In the early 1960s, Har Gobind Khorana, Marshall Warren Nirenberg, and Robert William Holley solved the code, showing that groups of three nucleotides formed codons that correspond to a certain amino acid, which link together to form proteins. Although there are still problems to solve, most notably the extremely complex nature of protein folding, these molecular biologists paved the way for a more thorough understanding of our genome and such grand projects as the Human Genome Project that has garnered much public attention. Khorana, Nirenberg, and Holley were awarded the Nobel Prize in Physiology or Medicine in 1968, the same year as Alvarez.

Meanwhile, high-energy particle physics research has lost much of its status as the premier science. The 7 TeV (trillion electron volts, a measure of power) Large

Hadron Collider at CERN has attracted a lot of attention, but a similar project in Texas was abandoned. The Superconducting Super Collider, which would have operated at 20 TeV, was cancelled in 1993 for budgetary reasons. Daniel Kevles described the affair in the 1995 edition of *The Physicists* in a new preface titled, "The Death of the Superconducting Super Collider in the Life of American Physics."[1] The federal government had seen particle accelerators as essential to national security, but "With the end of the Cold War, the disestablishment of American physics that began in the 1970s has been further extended; its claims to a share of the public purse are no longer taken largely on faith or dispensed with little obligation to accountability."[2] While particle physics is still an eminently respected field full of interesting problems, it is no longer the cool science. Edwin Slosson, the chemist who Alvarez read in his youth, argued in 1919 that chemistry—the manipulation of the materials around us—is at the heart of human endeavors. Physicists seemed to turn that idea on its head when they began manipulating matter and energy in unexpected new ways. In 2011, the *New York Times* published an article on the approaching possibility of synthetic life, quoting Nobel Prize winning physicist Freeman Dyson: "The ability to synthesize life will be an event of profound importance, like the invention of agriculture or the invention of metallurgy."[3] Molecular biology, despite its yet undelivered results, has taken the place of atomic physics as the

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[1] Daniel Kevles, *The Physicists: The History of a Scientific Community in Modern America* (Cambridge, Mass.: Harvard University Press, 1995), p. *ix*.
[2] *ibid*., p. *xlii*.
[3] Dennis Overbye, "'It's Alive! It's Alive!' Maybe Here on Earth," *New York Times* 27 July 2011.

king of the sciences. Scientists after the Second World War seemed to see it coming. As Alvarez wrote in 1972,

> It has often been said that the First World War was a chemist's war and the Second World War was a physicist's war. (That the Third World War might be a biologist's war was a frequently expressed fear in the late 1940's, but I am happy that I have not heard it mentioned for a long time.)

Thankfully, we have not had a Third World War and biological weapons have had very limited use with the notable exception of the Iran-Iraq war. However, he seems to foreshadow the rise of a branch of biology as the new trendy science after chemistry and physics.

There were trendy fields within physics as well. Alvarez described the centrality of particle physics in a 1967 interview:

> Nuclear physics, particle physics at the moment, is exciting because the best theorists are working in it and the people that you admire most in experimental physics are working on it, but if the theorists go away and leave it because there is no more experimental data coming out that they can analyze, they can quickly go to some other fields, and that will suddenly be the most interesting field, and you will go work in that. This is a completely new concept to the young physicists, because they've spent all their lives so far in particle physics and they think that's the only important thing in physics. I've been in a number of fields and I've found out that when the best people you know have moved suddenly into a new field, that's the most exciting field, because if you are working in the old field you are left behind; you are not good enough to be working with the first team over in this other field. That happened to me at M.I.T. and it happened again at Los Alamos. … I think that if you've been on the first team, you don't want to be playing in the scrubs.[4]

Part of Alvarez's success his sense of where to find the new hot field. As Rhodes put it, "Luie's life is a fascinating and wonderful life to tell, exactly because he went so many

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[4] Alvarez interview by Charles Weiner and Barry Richman on 15 Feb. 1967, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, session II, p. 25.

different lines at the same time. … He … wanted to be there, as he said more than one time to me, when things happened."[5] Eventually that drive took him out of physics. In his 1987 autobiography, he reported that "For several years I have divided my work equally among the study of global extinctions, the inventing of new optical products, and the writing of this book."[6] Even in retirement, the need to be in the middle of exciting things—to have adventures—continued, but it took him to dinosaur extinction work that was closer to evolutionary theory than hydrogen bubble chambers.

I began with a quote about Alvarez not wanting to explain what a physicist was at cocktail parties. Instead, he told people that he was a chemist so he would not have to explain what physics was and Richard Feynman did the same. In 2010, the *New York Times* asked physicist Sean Carroll the same question: "When you go to a cocktail party, do you tell people that you are a physicist? Some physicists won't." He replied, "I do! But I know what you're talking about. Whenever you say you're a physicist, there's a certain fraction of people who immediately go, 'Oh, I hated physics in high school.'"[7] The answer has certainly changed. Today, we can expect the public to know what physics is, but the physicist might choose not to say he is a physicist for different reasons. Physics still commands a great deal of respect, but the era of physics captured in Alvarez's career—a period where physicists could demand anything from government and industry—has faded. Alvarez captured the rise of physics; his diversions into other fields captured the field's relative decline, as well.

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[5] Rhodes interview by author, 20 May 2009, p. 44.

[6] Luis Alvarez, *Alvarez: Adventures of a Physicist* *(New York: Basic Books, 1987), p. 281.

[7] Claudia Dreifus, "Sean Carroll Talks School Science and Time Travel," *New York Times*, 19 Apr. 2010.

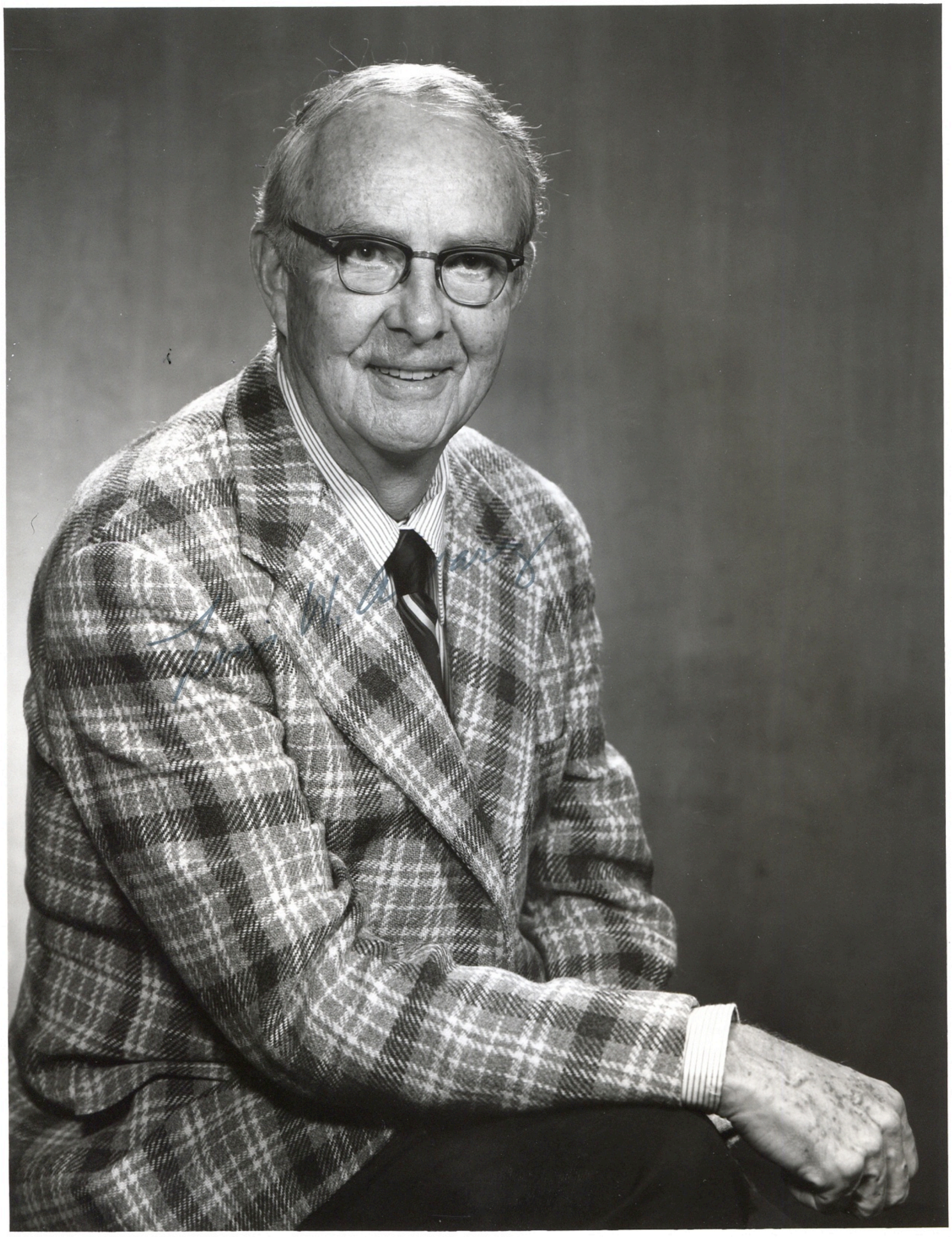

Figure 7.1: Luis W. Alvarez later in life.

Appendix: TEKS Textbooks

Textbooks in the Texas Essential Knowledge and Skills guideline (TEKS). Multiple editions are listed as multiple dates. Textbooks used that are not in TEKS are in the bibliography.

Abraham, Norman. 1967, 1969, 1973. Interaction Of Matter And Energy, An Introduction To Physical Science.

Adams, Charles Francis. 1896, 1909. Physical Laboratory Manual, For Secondary Schools.

———. 1908. Physics For Secondary Schools.

Adler, Irving 1966. Wonders Of Physics, An Introduction To The Physical World.

Ames, Joseph Sweetman. 1897, 1899. Theory Of Physics.

Andrews, Ernest John. 1903. Elements Of Physics.

Avery, Elroy Mckendree. 1878, 1881, 1885. Elements Of Natural Philosophy. A Text-Book For High Schools And Academies.

———. 1884. First Principles Of Natural Philosophy.

———. 1895. School Physics, A New Text-Book For High Schools And Academies.

———. 1897. Elementary Physics.

———. 1897. First Lessons In Physical Science.

Aylmer-Small, Sidney. 1943. Simplified Physics, A Clear Explanation Of Modern Science With Easily Made Apparatus And Many Simple Experiments.

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